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Mathematics and Experiment— Which First?

R. Duncan Luce

Individual Choice Behavior: A Theoretical Analysis. New York: John Wiley & Sons, 1959. Pp. xii + 153. \$5.95.

Reviewed by W. K. ESTES

Dr. Estes is Professor of Psychology at Indiana University, a well-known and learned psychologist of learning, who, among his many other activities, participated in reviewing Spence's Behavior Theory and Conditioning (Yale University Press, 1956) three years ago (CP, June 1957, 153-155). At that time CP described him as "an experimentalist who admires elegant theorems, cleancut experiments, and lucid papers [a pre-cognitive pun, that], and who says he hopes to live long enough to see a behavioral theory that fairly earns the title Newtonian—let alone Maxwellian." Well; here in the centennial year of Fechner's Psychophysik, we have something that claims to be nearer what Estes wanted to live to see. Do you suppose he might eventually make it all the way? He's only forty now, with half a life to go.

FOR the most part our knowledge and understanding of nature come from observation and experiment. But perhaps not entirely. The last century has seen a number of notable instances in which assumptions dictated, not by empirical considerations, but by the mathemati-

cian's sense of form, simplicity, and elegance have turned out to provide strikingly accurate descriptions and predictions of events in the physical world. The present volume reports the first extended effort to achieve comparable triumphs of rational (in the author's terminology, "axiomatic") analysis in psychology.

To appreciate this essay, it is essential that one distinguish it carefully from others that are superficially similar. Luce's work is not an attempt to substitute mathematics for experiment. That attempt was made once and for all by Herbart (for reasons that appear quaint today: "Psychology cannot experiment with men, and there is no apparatus for this purpose. So much the more carefully must we make use of mathematics!" *Lehrbuch der Psychologie*, 1816, trans. 1891, p. 4). The most generally accepted approach to quantitative psychology, associated historically with Fechner and Ebbinghaus, depends on the laboratory for the raw material of theory; in form and content its assumptions are dictated as directly as possible by experimentally demonstrated relationships. This, in the

eyes of most American experimentalists, is so nearly the only right way of operating that any deviation is bound to be viewed with suspicion.

A mathematical psychology analogous to mathematical physics—Luce's aim if I read him correctly—differs in spirit and method from all of the hitherto cultivated varieties. The theorist proceeds by seeking properties that behavior must exhibit if it is to be representable by a simple and pleasing mathematical structure. The resulting model conceptualizes the organism as it should be if nature has done a tidy job of engineering. Observation and experiment have a place in this scheme, but mainly at a relatively late stage when occasion has arisen to decide, by comparing alternative models with data, which intuitively reasonable scheme nature has actually adopted.

Although his orientation to the subject is relatively novel, Luce otherwise fits nicely the stereotype associated with influential contributors to quantitative psychology. The eminent quantifier typically receives his academic training outside of psychology, usually in physics or engineering. With this formality out of the way and some brief introduction to psychology accomplished, he aligns his sights on an important problem area in psychology and mounts an attack characterized by better mathematical tools than those used by his predecessors, minimal encumbrance by empirical data, and an exceedingly high level of aspiration. Then within an astonishingly short time, he puts forward a quantitative law characterized by a combination of engaging simplicity and sweeping generality—and writes a book.

Before his recent emergence in sheep's clothing as Professor of Psychology at the University of Pennsylvania, Duncan

Luce had earned a PhD in mathematics at MIT and had taught mathematical statistics and, in name at least, sociology, at Columbia and Harvard Universities. Not even Fechner, Thomson, or Thurstone was less hampered by early indoctrination in academic psychology. Luce's transition between disciplines was not quite so abrupt as it sounds in the formal *vita*, however, being marked by a period of participation in a "behavioral models project" and increasing fraternization with mathematically oriented social scientists and psychologists, some of whom are now among his colleagues at Pennsylvania.

THE candidate that Luce has put forward for the role of a "general law of choice behavior" appears well calculated to take its place with such predecessors as Weber's law, the tetrad difference, and Hull's product law for the combination of drive and habit. Luce's putative law is embodied in the first part of his first axiom, the heart of his formulation (indeed, in an earlier version, the whole theory). Its function is to establish relationships among the probabilities of choices from different sets of alternatives—relationships that are presumed to hold regardless of whether the chooser is a rat or a man, whether the alternatives are stimuli or responses, whether the setting is a psychological test or a T maze.

All this generality is a little hard to swallow, especially for the perennially disillusioned experimental psychologist. Thus it is not difficult to visualize the reaction of a sample of these professional cynics upon receiving, about two years ago, copies of the slender mimeographed volume in which Luce sketched his plan for bringing order out of randomness throughout the entire range of choice behavior.¹ This pamphlet, dubbed "the red menace" for reasons only in part associated with its scarlet cover (a feature preserved in the Wiley volume), proposed not only to fill this large order but to do so with a single axiom which was so simple in form that a schoolboy could apply it.

The gist of the "choice axiom" is the assumption that the relative probabilities of any two alternatives are independent of the total number of alterna-

tives available. Suppose, for example, that the listeners at a hi-fi concert are permitted to choose their favorite from the alternatives Bach, Chopin, and Debussy. Luce's axiom says that the ratio of the probability of choosing Bach to that of choosing Chopin is exactly the same as it would be if there were no Debussy records available and the choice were confined to Bach and Chopin alone.

Where this assumption came from one is not told. Certainly it did not arise in the manner of Kepler's laws as a result of patient sifting of masses of data. Luce cites as "the only published data which directly test axiom 1" F. R. Clarke's results on confusion matrices in articulation testing (1947). Interestingly, Clarke, working with J. P. Egan at Indiana University, had arrived independently at what he termed the "constant ratio rule" by a strictly empirical route during the same period when Luce was hatching an exactly equivalent axiom in the inscrutable way of the mathematician.

The principal justifications Luce himself offers for his choice axiom are two: First, he finds it to be "intuitively compelling." Secondly, he shows that "for situations in which pairwise choice discrimination is imperfect axiom 1 implies the existence of a ratio scale, i.e., one that is unique except for its unit, independent of any assumptions about the structure of the set of alternatives." The latter point brings us to the main theme of the book. The integrative effect which Luce suggests that his theory may exert on the many diverse disciplines dealing with choice behavior is intimately bound up with the all-but-universal ratio scale which "appears to be the formal counterpart of the intuitive idea of utility (or value) in economics, of incentive value in motivation, of subjective sensation in psychophysics, and of response strength in learning theory" (p. 3).

In demonstrating that the satisfaction of certain conditions upon the probabilities of observed choices necessarily entails the existence of a relatively unique numerical scale, Luce has presented a result of considerable significance. Just what this significance is will depend for each investigator upon his personal philosophy of science. If one considers measurement to be

an end in itself, then of course one's pleasure in Luce's achievement will simply vary directly with the number of empirical areas in which the conditions are found to be satisfied. If one takes the existence of a ratio scale as evidence for a unitary variable or process which somehow underlies and determines the observed behavior, then the exhibition of scales with identical mathematical properties in different areas of choice behavior might well occasion some real excitement.

Although Luce does not commit himself explicitly, the tenor of his presentation suggests sympathy for the latter view. In discussing the identification of the notion of response strength with the scale (termed the *v* scale) defined for any learning situation in which the choice axiom is satisfied, Luce argues that since the *v*-scale values uniquely determine the response probabilities, whereas the probabilities determine only ratios of *v*-scale values, "the *v*-scale may be more basic than the probabilities of response and that a learning model should be phrased in terms of *v*-scale values which indirectly alter the probability distribution" (p. 95). This is one of the few points at which I find myself differing sufficiently with Luce's views on method to provide material for an argument. A "basic" variable for me is not one that is empirically underdetermined but, quite the contrary, one that is overdetermined by a multiplicity of independent observables.

If one views measurement simply as a technique whose value in any given context depends on the extent to which it furthers the ends of description and prediction, Luce's contribution, although no less important, is of a somewhat different character. His demonstration cuts two ways. If his assumptions are satisfied in a number of situations that have hitherto been the province of the psychological scaler, this implies that behavior in each of these situations can be characterized in terms of a common ratio scale. But from the same premises it follows also that giving a particular mathematical function the label "utility scale" in one situation, "subjective sensation" in another, and "response strength" in a third adds nothing to the predictive power of the theory. All that can be predicted about behavior by means of these scales can equally well be predicted by means of a model couched in terms of relationships among probabilities of observed responses.

In some instances the scales may turn out to have a deeper significance. That such is the case for any particular substan-

tive area can only be demonstrated, however, by means of consistency checks in which determinations of the parameters of the scaling function are obtained from a number of different and independent experimental procedures (as done, for example, in S. S. Stevens' recently reported efforts toward "cross-modality validation" of scales erected by magnitude estimation: *J. exp. Psychol.*, 1959, 57, 201-209).

In over-all organization, Luce's theoretical presentation comprises a "basic theory" and three "applications"—to psychophysics, to utility theory, and to learning. The Basic Theory includes several theorems, the first of which—in some respects most important—establishes that if axiom 1 holds, all probabilities for choices from any set of alternatives are determined by the pairwise probabilities. Further, the pairwise probabilities are so constrained that the theory has considerable predictive power. Consider, for example, a set of ten alternatives with imperfect pairwise probabilities. Over 1000 subsets can be formed, and, if the choice probabilities were evaluated empirically for all of these subsets in a sufficiently extensive experiment, we would have over 1000 measures, none of which could be predicted from information about any of the others without the aid of a theory. According to Thurstone's Law of Comparative Judgment, information about nine of the pairwise probabilities would enable us to predict the remaining 36 paired comparisons. Applying Luce's axiom 1, information about 9 of the pairwise probabilities would enable us to predict not only the remaining paired comparisons, but also choices from any of the nearly 1000 additional subsets that could be presented. Considering both mathematical simplicity and data-reducing power, the constant ratio rule appears to be in a class by itself among all the models and analytic devices ever put forward in the entire field of psychometrics.

The chapter on applications to psychophysics is, for this reader at least, the most instructive in the book. Perhaps it is only to be expected that the approach of the mathematical psychologist would show to best advantage in this oldest and most worked-over corner of quantitative experimental psychology. In any event, such is the case. A series of empirical problems hitherto treated in quite disparate ways by different investigators (indeed, by different generations of investigators) is here analyzed in terms of the single set of notions comprising Luce's Basic Theory. Some of these analyses are quite enlightening, others provocative. In the former class is the elucidation of the relationship and differ-

ence between Stevens' scale of subjective intensity and Fechner's \log scale; in the latter is the derivation of a simple behavioral model for signal detection which offers an interesting alternative to the currently popular theory based on the "ideal observer" of statistical decision theory. These sections provide admirable examples of quantitative reasoning about psychological problems.

FOR psychologists, the most distinctive feature of Luce's work is his attempt to do something constructive about the rigid compartmentalization of quantitative psychology into learning and behavior theory, on the one hand, and psychometrics, on the other. This, rather than his skillful reworking of Fechner's



R. DUNCAN LUCE

mathematics or his breeding of new models in several already well-populated areas, may represent Luce's most important contribution.

The source of the compartmentalization lies, I suppose, in the omission of behavior from the subject matter of psychology during the formative period. Overt actions were of interest to the early experimental psychologist and his forebears simply as indicators of psychic events. The two pre-eminent classes of psychic events—ideas and sensations—evidently appeared to the introspectionist so qualitatively different as to call for quite disparate theoretical approaches. Herbart's prototype of modern behavioral models was formulated in terms of the competition of ideas. While developing a relatively unified and internally consistent treatment of learn-

ing and thinking, Herbart skirted the phenomena later to be subsumed under psychophysics with little more than a passing comment on "the psychological fact that we habitually carry with us a certain standard of measurement." These phenomena proved a natural point of departure for the experimentalists, whose industry in time demonstrated convincingly that human observers in making discriminations and judgments of various sorts do indeed behave as though they carried scales of measurement in their heads. Just how the observers manage to do this has remained unexplained. Consequently, while individuals interested primarily in measurement have proceeded with the business of constructing psychological scales, other investigators have tended to assign to the scale constructor a role like that of the honor student in a fraternity house—nice to have around for prestige but hardly expected to contribute to important activities.

Luce's strategy for promoting desegregation involves two steps. First he shows that the ratio scale defined by his axioms is identifiable with one already possessing some empirical standing in psychophysics. Then he undertakes to persuade the learning theorists that the same scale should form the basis of their models. The second phase of this program may run into more resistance now than it would at an earlier period. As recently as ten years ago, Skinner's negative answer to the question, "Are theories of learning necessary?" was based in part on the identification of mathematical theory in this field with a view which made observed behavior merely a symptom of variation in some underlying construct. The quantitative learning theories appearing during the last decade have, however, differed as a group from their predecessors on precisely this point. Their trend has been toward a statement of theoretical assumptions in terms of response probabilities rather than in terms of 'strength' or 'potentials.'

It is possible that Luce's major contribution toward unification may take a form somewhat different from the one he has himself envisaged. He has driven a Trojan horse up to the gate of psychometrics, and learning theorists will

be remiss indeed if they miss this opportunity to effect an entry. The occasional, desultory efforts on the part of learning theorists in the past to seek some rapprochement with psychophysics have been blocked by the lack of an univocal way of translating statements about stimuli and responses into statements about psychological scales. Once the conditions for the existence of a scale have been stated in terms of constraints on the probabilities of responses in choice situations, a clearcut problem is defined. If the conditions under which Luce's axioms do and do not hold can be derived from learning theory, then the curious observation that human behavior sometimes conforms to the strin-

gent demands of a ratio scale of measurement will have found an explanation in terms of independent concepts. Whether such a denouement is a Utopian ideal or an immediate practical possibility remains to be seen.

By way of over-all evaluation, I would like to remark only that Luce's enterprise has been conducted with a rare combination of imagination and technical skill. Whoever is seriously concerned with psychological theory will find it worth his while to examine the method and results of this study with some care *before* deciding whether the present situation is ripe for a 'mathematical psychology.'

Predicting Psychiatrists' Psyches

Robert R. Holt and Lester Luborsky, with the collaboration of William R. Morrow, David Rapaport, and Sibylle K. Escalona

Personality Patterns of Psychiatrists. (Menninger Clinic Monograph Series, No. 13.) Vol. I: *A Study of Methods for Selecting Residents.* New York: Basic Books, 1958. Pp. xiv + 386. \$7.50. Vol. II: *Supplementary and Supporting Data.* Topeka, Kan.: Menninger Foundation, 1958. Pp. xiv + 400.

Reviewed by ROBERT L. THORNDIKE

Dr. Thorndike is Professor of Education in Teachers College, Columbia University, and chairman of the Department of Psychological Foundations and Services. He has been at Teachers College since 1936 and is well known as an authority on tests and measurements, especially personnel selection and classification. He has just published, with Elizabeth Hagen, 10,000 Careers (Wiley, 1959).

WE have here the report of a specific program of research at the Menninger School of Psychiatry in Topeka, Kansas. The research had as its focus the development and evaluation of techniques for predicting success in psychiatric residency. Around the margin of this focal concern, the authors discuss in a more general way the nature of psychiatric training, the personality qualities that appear to be needed in being

effective as a resident and as a psychiatrist, and their own conclusions and convictions as to how the job of selection should be carried out.

In any program of research in picking men for a complex and extended course of professional training, definitely limited resources must be mobilized to attack an almost unlimited problem. The limitations are those of time, money, and the supply of subjects, limitations that impose upon the investigator a series of choices and compromises. He cannot study all possible predictors. He must be satisfied with a finite and often discouragingly small sample of cases. He cannot wait indefinitely for criterion measures or spend inordinate amounts of time getting them. An examination of the effectiveness with which available resources have been deployed provides one way of appraising the present project.

First one must set limits on the

types of variables that are to be studied as predictors. In general, Holt and Luborsky avoided the objective types of measures that have been used in large-scale educational, military, and industrial personnel enterprises, feeling that these measures were too atomistic and too superficial to be fruitful for so complex and variable a profession as psychiatry. Wechsler-Bellevue IQs, which were available for their subjects as a part of the regular admissions program, compared favorably in over-all validity with most of the more subjective devices. Scales of the Strong Vocational Interest Blank were also studied but with largely negative results. The main effort was devoted to unstructured procedures, such as the Rorschach, TAT, and clinical interview.

The authors were, however, commendably concerned about the variability of such unstructured procedures from one user to another, so that they directed a major effort toward developing 'manuals' to be used with these tests. These manuals were devised to provide an organized listing of positive and negative cues to be drawn from each of the tests. The lists of cues were developed in large measure empirically by study of the test performance of contrasting groups of high-rated and low-rated residents, and the manuals were then refined by tryout with subsequent samples. These attempts to make more reliable and consistent the procedures by which unstructured materials can be analyzed and evaluated were only partly successful. Though some of the manuals yielded quite high interjudge reliability, for others the consistency was discouragingly low. Moreover, the manuals that were relatively reliable yielded lower correlations with the main criteria of success than the unreliable ones. The authors believe that the procedures which they explored for bridging the gap between objective tests and unstructured materials have promise for the future, but they recognize that this promise cannot be realized until further cycles of hypothesizing cues and checking them on new samples have been completed.

A second major choice point is often generated by a limited flow of cases. In the present study, where data-gathering was spread out over six successive

classes—i.e., almost a six-year flow of personnel—the total number of individuals actually trained in the program was 238. An additional 46 were accepted but did not enter the program, while 172 applicants were rejected. When the total flow is so limited, a research worker must either freeze procedures at an early date or settle for a small final validation sample. If one freezes procedures early, one gives up the chance for exploratory and developmental work; but if one uses up his cases in the preliminaries, he is left with a limited group on which to test his final procedures and with correspondingly unstable statistics for the final test.

The authors of the present study made the second choice. They used the first four of their six classes in various ways to develop and refine procedures, primarily the procedures to objectify the cues drawn from projective materials. Only the last two classes were used for a final testing of the methods they had developed. It turned out that the number of useable cases in these two classes was only 64, and, because the design of this final study was quite complex and laborious, it was not possible to get results for even the full group of 64 for some of the measures. The most dramatic relationship between predictor and criterion, a correlation of .57 obtained for the final judgments of one judge after he had examined all the data available for each case, was based on only 38 cases. Though such a sample correlation clearly indicates a population value different from zero, the band within which the population value can be guaranteed to lie is still a distressingly broad one. One wonders, too, what the population is to which one is generalizing. Is it a population of judgments of groups of persons by *this* judge, or is it a population of judgments by many judges? It should be noted that a second judge, using the same basic data on the larger group of 64 cases, produced a correlation of .22. In general, the limited number of cases used in this final test results in a sense of inconclusiveness about the final results.

THE analysis of the final group in classes V and VI was viewed by the authors as a cross-validation study to

provide an independent and unbiased appraisal of procedures that had been developed with the earlier groups. This recognition of the crucial role of an *independent* validation of procedures and hypotheses that were generated from the analysis of the original data is gratifying. Even today, there are all too many prediction studies that try to test hypotheses upon the very data that have generated them. Nevertheless, cross-validation is a tricky business, especially when a number of distinct criterion components are being predicted. When, as in the present case, these components appear all to be minor variations on a common theme and to involve loadings on a single common factor, one must be careful to avoid (a) selecting the largest correlations as being typical of the set or (b) thinking of the different correlations in a set as providing independent confirming evidence about the validity of a predictor. In spite of their best intentions, the authors were guilty of each of these errors at times.

Furthermore, some of the data for the final experiment could hardly be considered cross-validational, for they were not a retesting of a procedure and rationale that had been worked out on a

previous group. The final experiment involved quite an elaborate design, in which several judges were used. Each judge started with a different segment of the data, which he scored analytically, rated globally, and organized into a personality picture. Then a new type of test protocol was coded and the process repeated. Finally, for two of the judges at least, all of the kinds of test and interview data were made available and the ratings carried out on the total protocol. The purpose of the design was to make it possible to appraise the contribution of each type of data, singly and as a supplement to other data. The hope was that this type of analysis would lead to an understanding of the source of valid inferences; but, except for the unanalyzed and incompletely described global ratings based on the complete complex of data, correlations were generally low and unimpressive. Validity did not seem to come from any one source, or to build up in any describable way. The isolated substantial correlations for the total complex of data (primarily for only one judge, as indicated above) cannot be considered as providing cross-validation of a technique whose validity had been suggested by an ear-



ROBERT R. HOLT (left) and LESTER LUBORSKY

lier part of the research. The technique had not been tried out with earlier groups, and there was no a priori basis for predicting that this rather than some other score or rating should have validity. It was strictly an a posteriori finding.

THE criterion problem provided some points of decision in this study as it has in many others. Except for the fact of withdrawal from the field of psychiatry—a fact which was closely related to and probably directly dependent upon the event of having been rejected by the Admissions Committee at the school and was meaningful only for the total group of applicants—the criterion data used were ratings of effectiveness given during the course of training by supervisors and by fellow residents. The use of a training criterion was almost inevitable, and the choice of ratings was a natural one. There may well be no better. Still, this criterion raises the possibility of contamination of predictor ratings by criterion ratings—especially as the exigencies of data-gathering and analysis apparently made some of the predictions actually postdictions.

The authors were aware of and concerned about the possibility of specific contamination induced by knowledge of some particular case. They present some evidence to show that it was not very important. Nevertheless, a more subtle and yet more pervasive contamination may have come from knowledge of the general flavor and basis of the criterion judgments on the part of the predicting judges; they may have known more than they legitimately should of the bases on which members of these specific classes were being judged high and low by those colleagues and associates who were rating them. Having this 'local knowledge,' they may then have been better able to use their comprehensive personality pictures to predict the ratings than would have been true if they were genuinely predicting over a period of time.

Aside from recognizing the methodological difficulties of such a project as this, and the sincere and well-conceived efforts of the authors to overcome them, what can one say about the results? Little more than that they are either

unexciting or inconclusive. The authors believe that the experience has given them some insights about selecting psychiatric residents, and they share this faith with the reader in the final section of their book. But such wisdom as there is in these proposals is an incidental by-product of the project, not something that flows in any direct way from the experimental results.

Selection research for such a group as psychiatric residents brings one face to face with many difficult and conflictful choices. Of some of them the authors

were aware in advance, whereas others became apparent to them as the project proceeded. Clearly, they ended their enterprise wiser—and perhaps sadder—than when they started it. They had grown in wisdom with respect to both the actuarial and the clinical aspects of selection research, and perhaps especially with respect to the constructive interaction between these two. The record of their learning can be instructive to their professional colleagues, both those whose orientation is primarily objective and those whose orientation is primarily clinical.

Science in the Clinic: a Predicament

Renée C. Fox

Experiment Perilous: Physicians and Patients Facing the Unknown.
Glencoe, Ill.: Free Press, 1959. Pp. 262. \$5.00.

Reviewed by ABRAHAM S. LUCHINS

Dr. Luchins is an experimental psychologist, who has gone into clinical psychology and taken his abiding faith in experimentalism with him. He is thus peculiarly well fitted to review the present book which deals with the difficulties of reconciling the scientific and clinical aspirations. His training was with Hadley Cantril and Max Wertheimer. He has his doctorate from New York University. He is now Professor of Psychology at the University of Miami in Coral Gables, Florida. He has published extensively in both his fields. His most recent books are Functional Approach to Training in Clinical Psychology (C. C. Thomas, 1959) and, with his wife, Rigidity of Behavior (Univ. Oregon Press, 1959).

RENÉE C. FOX is assistant professor of sociology at Barnard College and research associate of the Bureau of Applied Social Research, Columbia University. Under the auspices of the Bureau, she has since 1953 been engaged in research in the sociology of medical education.

This contribution to the growing literature of the psychology and sociology of medical research takes its title from Hippocrates' *Aphorisms*:

Life is short
And the art long
The occasion instant
Experiment perilous
Decision difficult.

The *experiment perilous* was conducted in a small teaching hospital affiliated with a prominent New England medical school. It involved an all-male ward for metabolic research, whose members agreed to act as research subjects for the hospital's Metabolic Group. The patients suffered from serious illnesses, whose etiology, diagnosis, and prognosis were shrouded in uncertainty as were some of the radical and potentially dangerous treatments tried on them, e.g., complete removal of the adrenal glands, hormone injections which sometimes resulted in psychotic episodes.

The author served as participant observer on this ward and during conferences of the Metabolic Group, making

most of the observations in two concentrated periods of five months each in 1951-1953 and in a short revisit in 1957. Other methods of analysis used included interviews, questionnaires, study of hospital records, and study of correspondence among the patient-subjects, the physician-researchers, and the sociologist-author.

Problems and stresses encountered by the participants in 'experiment perilous' are documented with verbatim reports which illustrate the reactions on facing the unknown. The point of view is a fusion of the psychological and sociological orientations, but with emphasis on the latter; e.g., more attention is paid to 'socially patterned' ways of meeting stress than to more individualized ways assumed to be largely personality-determined.

Methods of meeting stress were strikingly similar for the patient-subjects and the physician-researchers. They included strong intergroup and intragroup ties which continued outside of the hospital. (The evidence presented, although indicative of personal interest by physicians in patients, does not seem fully to support the author's impression that patients were treated almost as professional equals.) Within each group, stress was relieved by grim or 'graveyard' humor and by wagering on the outcomes of someone's illness or on a particular aspect of the research. The reactions are similar to those described in some accounts of soldiers in combat and persons involved in peace-time disasters.

PHYSIANS of the Metabolic Group "often felt quite disturbed over the inadequacy or uncertainty of their knowledge, their inability to diagnose or definitely improve the clinical status of many of their patients, and the difficulties they experienced in trying to reconcile their clinical responsibilities with their responsibilities as investigators." The reviewer wonders to what extent the latter difficulties—and analogous difficulties which sometimes confront clinical psychologists and others who occupy the dual positions of practitioners and researchers—are contributed to by certain conceptions of scientific research. The belief that theories are objectives rather than primarily tools of

science, the belief that research must be theory-centered rather than patient-centered, and the belief that it is somehow less scientific to deal with a particular case in its own right than to contribute to 'general knowledge'—all these attitudes can make for conflict between the requirements of the clinician and the investigator. It would therefore have been desirable to have more information about the physicians' specific conceptions of scientific research.

The reported clash between the role of the scientist and the clinician raises the question as to what extent the conflict is due to specific assumptions and attitudes arising from the education and cultural values of the physicians rather than from an inherent antagonism between 'science' and 'life.' Science has often been depicted as a dehumanized activity, a modern Baal to which its believers cold-bloodedly sacrifice what ordinary people hold most precious. Insufficient emphasis has been placed on the fact that science is a human activity that may be carried out by humane individuals.

The book is of interest to those who are called on to do 'practical' research. It may help them to realize that scientific research, as described in courses and textbooks, is not necessarily rigorously related to scientific endeavor in a life situation and that an experiment need not be planned and performed as suggested in texts on method but may be a distillate of a mish-mash of blind trial and error, irrational hunches, as well as rational deductions. Methodological neatness does not guarantee creativity or great discoveries. Frustrations experienced by those participating in 'experiment perilous' may help to prepare an investigator for frustrating experiences in his own research. He may even be led to wonder whether the ideal of research with which he has been inculcated, even though theoretically desirable, is practically possible.

There is also a question of whether the author entirely achieved her intention to portray the ward as a dynamic social system. So bright is the spotlight thrown on the main performers—the patient-subjects and the physician-investigators—that one gets only a dim view of the stage on which they perform and

of the other dramatis personae. It would have been helpful, as a background for viewing the main performers, to have had more information concerning the social structure and processes of the ward, of other people on the wards, of daily ward activities besides those pertaining to the research and related treatment, and of the relation of the ward to other units of the hospital and to the nonhospital community.

There is no question that in this very readable chronicle of the human aspects of research, the author succeeds in her intentions of making the hospital world better understood and of revealing man's reactions to stress on facing the unknown.

When to Choose Choice

Herman Chernoff and Lincoln E. Moses

Elementary Decision Theory. New York: John Wiley & Sons, 1959. Pp. xvi + 364. \$7.50.

Reviewed by R. DUNCAN LUCE

who is now Professor of Psychology at the University of Pennsylvania. He was born a PhD in mathematics at the Massachusetts Institute of Technology almost a decade ago. He was brought up as a psychologist by George Miller, "Lick" Licklider, Walter Rosenblith, and Alex Bavelas. He co-directed Bavelas' small-group laboratory, exposed himself to Paul Lazarsfeld at Columbia, taught sociology and mathematical statistics there, met Harold Raiffa and wrote with him *Games and Decisions* (Wiley, 1957), experienced the Center for Advanced Study in the Behavioral Sciences, found himself in the Department of Social Relations at Harvard, where he wrote *Individual Choice Behavior* (Wiley, 1959; CP, April 1960, 5, 113-116). As to what he writes next, wait and see.

To an unprecedented degree, psychologists have ingested both clas-

sical statistics and a rigid policy concerning its use in scientific inference, so they, more than most other scientists, should keep abreast of alternative statistical formulations. Decision theory is just such an alternative, and here is its first elementary text. The authors are, respectively, Professor and Associate Professor of Statistics at Stanford University. Chernoff has contributed major papers to the basic literature of the field. Thus, both on grounds of importance and authority, the book cannot be lightly dismissed by any of us, research scientist or teacher.

In science proper, classical statistics has been used to treat three related problems: hypothesis testing, goodness of fit, and estimation. Throughout, the measures used to evaluate data are error probabilities—the ubiquitous significance levels and, less often, the probabilities of type II errors. But this scheme is not without its difficulties. First, it is not always certain which probabilities to calculate, especially in complex problems. Second, it is unclear what they mean in practice, because we suppress so many 'nonsignificant' results. A published .05 finding may well be judged significant if no similar studies have been buried in files; it hardly can be if 19 other laboratories have gotten nonsignificant results.

Be that as it may, in applied science certainly, and some feel throughout science, more is needed than these probability statements. One's client is usually concerned with choosing an action, not with cautious .01 statement. Statistical decision theory attempts to formulate his problem in this way. Given the possible actions that he can take, the possible states of nature that differentially affect the outcomes of the actions, and data partially revealing which state obtains, which action is in some sense optimal? The problem is not, however, well formulated until the importance of the various possible outcomes has been stated by the client. Herein lies the major difference between decision theory and classical statistics.

The degree to which the new approach should displace the old depends upon the extent to which scientific issues can be usefully recast as action problems. Some statisticians, apparently including

these authors, believe that they all can be. Others, including this reviewer, question this view, arguing that when a finding is published one can hardly know the uses to which it will be put, and therefore it is unreasonable to attempt to assign costs to errors. This group does not deny the problems inherent in our present practices, but only questions whether decision theory is suited to pure science. Nonetheless, there is little doubt that decision theory is superior to the classical formulation when actions are at issue and it sheds considerable light upon the classical methods. Surely most psychologists should be equally familiar with both approaches, should they not?

The next question, then, is whether this book can be used in an introductory course in statistics. The level seems appropriate, albeit sophisticated. The book builds from high-school mathematics by clear and detailed discussion, interlarded with numerical examples; the authors are not hesitant, however, in their development of abstract ideas or their use of symbols. It is too bad that their style is so undistinguished and, at times, even tiresome (especially in some of the examples), but it is nevertheless always lucid. Their coverage of decision theory is excellent. Beginning with descriptive statistics, probability, and random variables, they pass to utility theory, which is needed to justify using numerical evaluations of outcomes, and then to the formulation of the basic decision problem. This is then reduced to a consideration of Bayes strategies and the uses of Bayes theorem. The final chapters examine the decision-theoretic analysis of hypothesis testing, confidence intervals, and estimation. There is a little too much material for one semester, and probably not enough for two; however, were one to teach both this and supplementary classical statistics, the two would make a neat two-term package.

No teacher of elementary statistics can now omit decision theory on the grounds that there is no suitable text; his reasons will have to be more positive.

The Administrative Struggle

Melville Dalton

Men Who Manage: Fusions of Feeling and Theory of Administration. New York: John Wiley & Sons, 1959. Pp. xii + 318. \$6.75.

Reviewed by CHRIS ARGYRIS

who is Associate Professor of Industrial Administration at Yale University. He has written over half a dozen books on human relations in industrial and other organizations, of which the earliest was *Executive Leadership* (Harper, 1953) and the latest will soon be *Theory and Method in Diagnosing Organizational Behavior* (Dorsey, 1960). He opposes the conceptual simplification of organization for the sake of economy of thinking and insists that organization must be regarded whole and that complexity and contradiction are essential to any human structure.

IN this volume Melville Dalton, Associate Professor of Anthropology and Sociology, University of California at Los Angeles, provides rich descriptions of the inner workings of four American firms.

After charting the formal organization of a firm, Dalton shows how in reality it is altered to fit the shifting social relations, the emergencies of work, and the power struggles among executives. Even more fascinating are his examples of how the line managers continually force the staff to justify their existence and yet simultaneously resist the changes the staff recommends. Vivid descriptions follow of how the staff, feeling undercut and having low status, strives through informal means to please the line, and of the power struggles that surround change.

Dalton scrutinizes the process of upward mobility. Along with the official criteria of selection (ability, drive, etc.), he finds equally important abilities to utilize and aid the existing cliques, to control dangerous ones, to be loyal to one's sponsor, to be expert in power



politics, to hold membership in the Masons and the 'right' yacht club, to have a proper ancestry, and to come from an accepted ethnic group.

One may question how generalizable are the results of these 'case' studies. Testing hypotheses and formulating generalizations with statistical significance are not Dalton's objectives. He wants to present the social scientist and the practitioner with data regarding the 'compromise among key individuals and groups in rational organizations' and to describe the world of the organization as it is seen from the inside at various points and levels. He accomplishes these objectives without belittling or chastizing any group that he portrays.

CONFLICT, he concludes, always exists and is not necessarily bad. It can help to challenge the skill of individuals in interpersonal relations. Other challenges come from 'beating' the iron law of American bureaucratic practice ("there's always a way to get around the rule—look for it").

Dalton's final word is that the individual's problem in an organization is not horrible conformity. Rather he should have the freedom to show himself as he really is. "Those who mistake surface conformity in organizations for total conformity . . . should refocus to concern themselves with the ethics of protective coloration among thinking animals."

Some readers may conclude that Dalton's method of participant observation, 'informal' scrutiny of company files, etc. is 'soft-minded.' If this is their conclusion, I wonder how else they would obtain such 'living' data. Sometimes a researcher finds it necessary to be soft-minded in method in order to study 'tough-minded' problems. I do indeed raise a question about the practice of interviewing subjects and not telling them clearly the researcher's intentions, but, having raised that question, I find that I cannot suggest an alternative.

It would, however, be unfortunate if eventually the key insights in the study are not recast as hypotheses to be tested so that the generalizations can be more readily fitted in the behavioral organizational theories as they evolve.

"When I Use a Word," Said Humpty Dumpty

George Mandler and William Kessen

The Language of Psychology. New York: John Wiley & Sons, 1959. Pp. xviii + 301. \$6.75.

Reviewed by ROBERT L. ISAACSON

Dr. Isaacson is a member of the Department of Psychology at the University of Michigan, where not long ago he obtained his PhD. He has been involved in research in audition and has studied problems of motivation by both physiological and projective techniques. His interest in language arose under the stimulation of E. L. Walker and George Miller.

THE only known treatment for a chronic reflective interest in the philosophic bases of one's science is a symptomatic one: administration of large amounts of knowledge. This is hardly a cure, for the patients are insatiable, but it is the best we have. George Mandler and William Kessen have made the remedy very attractive for psychologists. They have extracted many of the best contributions of the philosophers of science, added some original ideas, and made the compound meaningful for psychologists by embedding it in a medium of behavioral data. The terminal product fills a gap in the psychophilopedic literature. It may safely be prescribed for scientists in all stages of the illness. Even those in the most advanced stages can profit from the psychological analysis of the philosophical processes, which is probably the most unusual contribution of *The Language of Psychology*.

As with language itself, the book is divided into two parts: vocabulary and grammar. The first section takes a hard look at the words we use when we speak as scientists, while the second deals with the problems of the nature of scientific theories. Much of both sections follows from the work of Hempel, Quine, and

Wittgenstein, and need not be reviewed here. However, part of the value of the book is that it presents the work of these men in a readily available and readable form, and Mandler and Kessen are careful to give them their appropriate referenced credits. Since the philosophical contributions are presented in a psychological context, we can see immediately their relevance for our everyday activities.

What words should be used in scientific theories? Various rules have been proposed, but Mandler and Kessen conclude that all of them suffer from being unnecessarily restrictive. They are all seen as special cases of one more general criterion: invariant usage. A word should have both intraindividual and interindividual consistency if it is to be useful in science. It does not matter whether the word is in response to "real world objects" or to other verbal stimuli; your colleagues should use the words as you do, did yesterday, and will use them tomorrow. Previously proposed criteria have been based on phenomenal or physical consistencies, and I am sure that psychologists will be disposed to accept the authors' contention that these must be inferred from the invariant use of terms.

How do words come to be used consistently? Some words in the vernacular achieve a high degree of consistency. Prosaic though it may seem, the authors explain such invariant natural words through consistent reinforcement of certain of their occurrences.

Words may, however, achieve invariant usage in scientific discourse through the use of real, nominal, and operational definitions or through partial reduction-

sentences. The value of the public communication requisite for scientific activities is only as great as the reliability of the words used in the communication.

SCIENTISTS communicate facts and theories to each other. We spend a great deal of time evaluating and criticizing the assumptions underlying another's theories, but tend rashly to accept his data, his facts, as reported in the protocol language. By careful analysis of protocol sentences, Mandler and Kessen force us to realize that these basic data sentences usually contain a number of auxiliary hypotheses which themselves require empirical confirmation. For instance, when we use the phrase "very compulsive by Rorschach test," we have assumed certain things about the stability of human behavior and the relation between perception and behavior. Such statements do not find ready acceptance by all psychologists. The greater acceptability of protocol statements about rats in a maze stems, in part, from the greater acceptability of the auxiliary hypotheses involved. From consideration of these hidden assumptions we are forced to reject any naive view of facts as inviolate truths about nature.

Mandler and Kessen suggest we should not be impatient with the progress of psychology. The early stages of all sciences are marked by a certain vagueness of word usage. This condition, however deplorable in a philosophical sense, may be of heuristic value for the development of the science. The final evaluation of any theory must be made on the basis of its over-all empirical usefulness.

The two authors come from different areas of psychology. George Mandler's name is associated with research in motivation and anxiety, while William Kessen's areas of principal interest include the behavior of children and learning. Both received their doctorates from Yale University about six years ago. Kessen is still at Yale, but Mandler moved to Harvard and is spending the year at the Institute for Advanced Study in the Behavioral Sciences.

The book offers knowledge for those who will accept it. Acquaintance with its contents would make us all more critical and discerning of the theoretical

developments the future will bring. The authors hope that their efforts will prove to be "useful," but the volume is not, and should not, be taken as a guide to the construction of "good theories." *The Language of Psychology* can be only as useful as any knowledge is to a prepared and creative mind.

Some Antecedents of Criminality

William McCord and Joan McCord, with Irving Kenneth Zola

Origins of Crime: A New Evaluation of the Cambridge-Somerville Youth Study. New York: Columbia University Press, 1959. Pp. xviii + 219. \$6.00.

Reviewed by A. ARTHUR HARTMAN

who is Director of Psychology and Research in the Psychiatric Institute of the Municipal Court of Chicago. Twenty-five years ago his first professional work was with the Institute for Juvenile Research, and since then he has worked successively with the Joliet-Stateville Penitentiary and the Cook County Juvenile Court. He is a past-president of The Illinois Academy of Criminology.

ORDINARILY a book jacket announcing that "it is impossible to overrate the importance of this contribution" would be discounted as publisher's license. In this case, however, the claim is consistent with the spirit, if not the substance, of the book itself, which expresses a sense of discovery and a degree of conviction rare in criminological research.

The authors, currently in the Sociology Department at Stanford University, migrated recently from their Harvard environs. William McCord taught social psychology at Harvard, and his wife, Joan McCord, was Co-Director of the Cambridge-Somerville Research Project. Together with Irving K. Zola, a Harvard Fellow, they have analyzed statistically the case records of a pio-

neer experiment in preventing delinquency.

In the late 1930s, Richard Clarke Cabot conceived of an experimental program utilizing individual counseling and the services of social agencies. A treatment group of 325 boys, matched for delinquency prognosis with a similar control group, was selected from the cities of Cambridge and Somerville, Massachusetts. The program terminated in 1945, having weathered a world war and other major vicissitudes. A subsequent three-year follow-up by Edwin Powers and Helen Witmer revealed that about 40% of the boys in both treatment and control groups developed later criminal records.

The McCords address themselves first to a systematic ten-year follow-up of the Cambridge-Somerville Project. After a clear critique of its assumptions and fallacies, they confirm earlier opinions that the Project had failed in its treatment objectives. They demonstrate, however, that the failure was no reflection on Dr. Cabot, whose original criterion of an intensive therapy program had only minimally been realized.

THE second section of the book, dealing with specific determinants in crime, is offered by the authors as their principal contribution. Their major thesis is that the causes of crime lie in early pathological family influences. Stated thus baldly, the book appears to follow a long tradition of similar reports from William Healy to the sustained research of the Gluecks, whose latest book, *Predicting Delinquency and Crime*, is just off the press. The McCords adduce a wealth of ingenious comparisons of various home influences upon crime. A few examples: parental neglect, more than parental cruelty, is likely to lead to criminality; maternal passivity is conducive to crime, while paternal passivity is not; the influence of one factor, the mother's attitude, "may increase a boy's chances of crime by almost 300 per cent."

While the McCords' procedure is typical of studies on the differences between criminal and noncriminal groups, the essential comparisons are not actually completed. Conclusions are primarily based on analysis of the treatment

group, since data on the control group, the authors indicate, were not sufficiently reliable or complete. Thus the findings here are in the nature of a predictive test reliably determined for one group, but still not validated upon other groups.

The book uses the term *causation* in a very free sense equivalent to that of simple sequence, ignoring John Stuart Mill's basic caution about *necessary* relationships: There is a constant inference of universal causation which can hardly be accepted without more proof of the authors' assumption that Cambridge-Somerville males are representative of American males generally. (Does Harvard University exercise no unique influence?)

The McCords write in a positive, persuasive style, interesting except in the intricate tabular comparisons. At the drop of a significant percentage, they reach at once for interpretations and generalizations. Perhaps they are too much absorbed in their data. For example, they conclude at one point that the most significant factor influencing the *type* of crime is the mother's personality; thereupon they add, "Neighborhood and intelligence have practically no bearing on the choice of crime," a statement which any self-respecting bank embezzler would be quick to refute.

Evidently the authors have in mind a wide, even popular, audience. Their interpretations will likely find a sympathetic response from those who concur in a concept of crime as a pathological process. Sophisticated readers are more likely to be intrigued by the findings but to remain skeptical of many of the generalizations.

A large-scale longitudinal study which covers a ten-year period is almost a unique accomplishment in itself. As a contribution to understanding some of the early family influences associated with later criminality, this book does, indeed, add important data. Yet it falls short of its more lofty aspirations to originality and significance as a scientific break-through in the prediction of criminal behavior.

Still Some Doubt in Lesbos

Richard C. Robertiello

Voyage from Lesbos: The Psychoanalysis of a Female Homosexual. New York: Citadel Press, 1959. Pp. 253. \$4.00.

Reviewed by EVELYN HOOKER

who is Research Associate in Psychology at the University of California at Los Angeles and Principal Investigator, with support from the National Institute for Mental Health, of male homosexuality in the Los Angeles community. A woman, of course, can work with male homosexuals without being suspect of deviation herself. She is now writing two books: Homosexual-Heterosexual Development: a Psychological Comparison of 60 Men, and Homosexual Community. Ever so much she wants in this area to "supplant myth and fiction with fact."

So little is known about the causes, nature and possibility of 'cure' of homosexuality, either male or female, that a book concerned with any of these aspects of the topic deserves, and will probably receive, attention. The author, Dr. Richard C. Robertiello, is a psychiatrist-psychoanalyst who, in addition to private practice in New York City, holds a number of important posts, including those of Chief Psychiatrist of the Long Island Consultation Center and Supervising Psychiatrist of the Community Guidance Center.

In simple, readable, and unpretentious language, except for the theoretical interpretations, he describes the essential features of Connie's (the lesbian's) history before entering analysis and the vicissitudes of her experience in the four years of analysis. The dream progression, a large part of the book, is fascinating. The voyage is a stormy one as Connie struggles with heterosexual wishes which evoke anxiety and fear, and, through cycles of recurring homosexuality, is finally able to renounce it and achieve heterosexuality. As a case

history purporting to be a complete report of four years' analytic work, the account is much abbreviated.

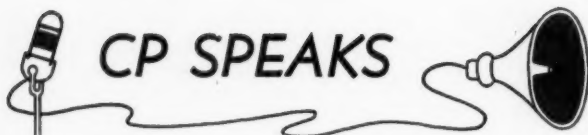
For the reader who does not take psychoanalytic constructs as self-evident truths, the interpretations leave much to be desired. That the analyst has thoroughly indoctrinated the analysand with his theory is evident in the ease with which such terms as *oedipus complex* and *penis envy* are used by both. This reviewer's patience was sorely tried by the frequent gaps between evidence and explanation.

The most serious additional criticism of the book is that, while ostensibly a case history of a single homosexual girl, the text does, in fact, become a treatise on homosexuality. Generalizations such as the following should be viewed with caution and doubt: (1) "homosexuality is the symptom of an illness;" (2) it is caused by "certain traumatic experiences in childhood;" (3) "it is not a matter of choice but of compulsion."

That all homosexuals are ill has been seriously challenged by a highly respected British psychiatrist (Dr. Desmond Curran, 1957) and by the Report of the Wolfenden Committee to the British Parliament (1957). That homosexuality is caused by traumatic experiences in childhood is a belief which is in contradiction to the prevailing opinions of social psychiatry (about mental illness in general) which "lay emphasis upon current social roles and social environmental contingencies" (Rioch, 1959). That it is not a matter of choice but of compulsion is an assertion based on theory and the observations of clinical patients who constitute but a small and possibly atypical proportion of the total homosexual population.

Perhaps Dr. Silverberg is right when he states in the introduction that this book may "fill a need of candidates in psychoanalytic training and recent graduates from such training." For the student who has learned to rely on rules of scientific evidence, it will be of little help in understanding this complex problem. Indeed, it may be misleading when its theoretical assumptions are made as statements of fact.





CP SPEAKS

NOTHING-BUT AND SOMETHING-MORE

THE reason that science—every science—has to be regarded as a social institution is not only that its ultimate sanction lies in its use to man, that it was the threat of the sputniks that moved America at least one just noticeable difference away from its previous level of anti-intellectualism, but also that scientific 'truth' depends upon value judgments, and that the 'good' is what people—any in-group large enough to have a culture or a creed—want. When automation has made human beings unnecessary, science will be different. It can go on observing, recording, grinding out consequent inferences, but the decision as to what dimension of events to observe (all description is analytical), and at what level of automated confidence to stop grinding on this so as to begin on that, will depend on the scientific public opinion of all the grinding machines, as polled by a special automatic value-assessor. You cannot, however, undertake polling unless you have variability, but undoubtedly there would be individual differences among these human surrogates simply because there'd be accidents in their reproductive process, when one machine (or would it still take two to turn the trick?) begets another. Every scientific theory is just the best opinion of what it is a good idea to believe—for the date of the belief, for the place where the believing happens, and sometimes for the in-group that first got the belief in circulation. That is how social a science is.

Now a journal of reviews and criticism like *CP* runs head-on into the value problem. The reviewing can, of course, come somewhere near 'objectivity,' stand up as a kind of filtered photography which is a fair indicator of the original. (Oliver Wendell Holmes,

Sr., remarked in 1861 that David Brewster's invention of the 'parlor' model of the stereoscope might make foreign travel unnecessary, but the Europeans do not nowadays find that it worked out that way.) The best description is not the thing itself, and a scientific fact is not the event itself but an abstracted generalization about a class of events. Let reviewers try to be as objective as they can, yet two reviews of the same book are likely to differ enormously. That is why you have always to take the single review as idiosyncratic. Reviewers are idiosyncrats.

Just so objective can description be, but there is also the criticism. *CP* promotes criticism, which necessarily is even more idiosyncratic than description, but *CP* also tries to control criticism just a little. It says in its *Comments to Reviewers* (of which every reviewer gets a copy): "Always try to see how nearly the author has realized his own aspiration, whether you approve of the aspiration or not." That is good advice, and certainly a fair review inquires whether or not the author has achieved his goal. But what if the reviewer disapproves of the author's goal? What if their respective idiosyncrasies, the reviewer's and the author's, include incompatible values, each holding to his own beliefs with a slightly fanatical solicitude? Is the reviewer to keep silence though his conscience be outraged—even when he knows that the author would tell him that he had the wrong kind of a conscience? Surely not. The reviewer's duty is to maintain his own integrity, to say whether the author accomplished what he intended and then to add his judgment of the intention. Criticism operates in a free market and the reviewer must expect a countercriticism in which his own credo is disparaged.

Nowhere does this contradiction show

more clearly and frequently than between the 'Nothing-But' reductionists and the 'Something-More' people who have no single name to cover them. The NB people are the positivists, the physicalists, the operationists, and they are opposed by SM people who will not tolerate the restrictive canons of an ironclad reductionism, reduction to sets of rules that shackle the free play of the scientific imagination. Phenomenologists, intuitionists, they might sometimes be called. They are people like William James, Wolfgang Köhler, and Gordon Allport, if you can manage to see in what way these three are alike. The important dimension is from rigidity to freedom. The experimentalist and the philosopher represent the two poles, but there is lots of good SM science that is not philosophy.

So every now and then *CP* gets one of these slightly fanatical Nothing-But reviewers for a book by a Something-More author, and the NB reviewer complains that the SM author's reasoning is not rigorous—he has not experimented when he might, he had too few cases, he relied on inspection when he should have used statistical analysis, he used the method of agreement but not the method of difference (which is to say he had no controls). Should the reviewer keep silent when his scientific credo is violated? No, *CP* says, he should not. Science does not ask him to violate his own integrity. But he must expect rejoinders that remind him that he is nothing but a Nothing-But-er, that, if he eternally keeps his nose to the grindstone, he will never cut a wide swath. The SM author gets the broader view, nor should the NB reductionist forget that most of Freud's contribution to psychology was of the SM variety. Some NB experimentalists know this fact and hold Freud's difficultly achieved conclusions in low esteem for that reason. And right there you have the issue: Freud and Helmholtz. Which would you rather be? *CP* says psychology wants both. They can complain about each other in *CP*'s pages and psychology will be the broader for these divergent efforts and the better for the clarifying influence of perpetual dissent.

Thus *CP* becomes an organ of dissent. It has space for idiosyncrasy—not all

but some, for the Editor has his limiting credo too. Why do so few people realize how dissent promotes progress? Because—so *CP* thinks—so many are in some degree fanatics. All right. Fanaticisms too—many divergent incompatible fanaticisms—promote progress.

MERCURIES AND SOWS' EARS

AFTER he had found out what it meant, *CP's* Editor thought that Pythagoras' dictum on education

NON EX QUOVIS LIGNO MERCURIUS FIT

would be a wonderfully good motto for the staff room of a Department of Psychology where the applications for graduate study are worked over and the most promising candidates selected. Unfortunately, however, the graduate student is not easily going to discover the meaning. "Not of any log is a Mercury made." "A Mercury" is not Mercury himself but a statue of the god, and statues of Mercury had to be made of boxwood. You could say, "Only boxwood makes a Mercury." But *lignum* is firewood found lying around, common stuff, not boards and timber which are *materia*. "Not from any hunk of wood is a Mercury made." Still the graduate students would not then realize what a Mercury is. You would be safe to say, "Not from any hunk of wood is a god fashioned." Just at this point, however, some believer in the vernacular is sure to come in with the remark: "Why go to the Greeks, when you have the Irish?" Jonathan Swift had the same thought:

YOU CAN'T MAKE A SILK PURSE OUT OF
A SOW'S EAR

How beastly the vernacular is! Harvard just might make a Mercury out of some hunk of Eli wood, but it wouldn't want one of Yale's sow's ears for any of its silk purses.

HYPNOSIS

CONCERNING three recent books on hypnosis Dr. Robert W. White has this to say.

The recent strong revival of interest in medical hypnosis seems to be no passing fad. This is attested by the appearance of two second editions within six years of the

first. *Hypnosis in Modern Medicine*, edited by Jerome M. Schneck (Charles C Thomas, 1959), a valuable compendium describing applications to internal medicine, surgery, dentistry, psychiatry, and several other specialties, has been updated by the addition of a 60-page review of pertinent literature since 1953. *Time Distortion in Hypnosis*, by Linn F. Cooper and Milton H. Erickson (Williams & Wilkins, 1959), originally published in 1954, is substantially the same except for an additional chapter reporting some work on time condensation, a 'distortion' not included in the earlier experimental and therapeutic program. The Institute for Research in Hypnosis (New York, 1958) has published in pamphlet form a series of symposium talks entitled *Introductory Lectures in Medical Hypnosis* (Margaretta K. Bowers, editor), still further evidence of the zeal to recover, at a much higher scientific level, some of the medical prestige enjoyed by hypnotism in the days of Bernheim and Charcot.

BOOKS TO COME

THERE is a book due sometime, maybe next fall, that will tell people who do not know what it is that psychologists do—in fact, that might be the title of the book: *What Psychologists Do*. Dr. Wilse B. Webb of the University of Florida is the editor and has ten other psychologists, including some very well-known persons, to tell people, mostly prospective and actual graduate students, what it is to be an industrial psychologist, a school psychologist, a university psychologist, a clinical psychologist, a psychologist in private practice. Students just do not very often know what it is like to be a psychologist, and indeed the book might also accomplish a heuristic contemporary definition of psychology. There'll be a general introduction and also a chapter on what psychology is not—not written, however, by a distinguished professional

nonpsychologist. Henry Holt is the patron of this undertaking.

DECISION-making keeps right on stimulating thinking. In May, McGraw-Hill will publish *Bargaining and Group Decision Making: Experiments in Bilateral Monopoly*, by Sidney Siegel, an experimental social psychologist at Pennsylvania State University, and Lawrence E. Fouraker, an economist of long standing at the same institution. Bilateral monopoly is the union leader and the representative of management, each monopolizing one side, the professor and the dean, the cleaning woman and your wife. This is meant to be a firm marriage, the rigor of economics in theory with psychology's reassurance by experimental test. The book ought to be good. It received the \$1000 prize of the American Academy of Arts and Sciences for social science, and the grapevine says that the competition for these prizes was enormous.

SOMEWHERE along toward the end of this year Holt is going to publish T. M. Newcomb's *The Acquaintance Process*. What's it about? It's social acquaintance (not the epistemologist's acquaintance with). Newcomb—as far as *CP* can make out—decided to take a small college, fill it with strangers, and watch them change their attitudes and beliefs as they became a group. Ultimately he had to settle for less than a college, Project House, a melting pot, where you got free lodging for letting yourself melt gradually under recorded observation. Idea conceived: 1943. Grant achieved: 1953. Data reaped: 1954–1956. Analysis, comprehension, tabulation, Englishation, these on up into 1959. Sounds like fun. *CP* believes that not too sober a group was evolved with TMN around as *deus ex societate*.

—E. G. B.



The most important essential of every literary composition, be it poem, treatise, history, tale, or aught else, is unity. Upon this depends our power of viewing the composition as a whole, and of deriving pleasure from the grasp that we thereby obtain of it, as well as from our perception of the harmony and mutual adaptation of the parts, the progress and conduct of the argument, and the interconnection of the various portions with one another.

—GEORGE RAWLINSON

Atomized Personality in Hyperspace

J. P. Guilford

Personality. New York: McGraw-Hill, 1959. Pp. xiv + 562. \$7.50.

Reviewed by RONALD TAFT

Dr. Taft is an Australian, with a BA from Melbourne, an AM from Columbia, and a PhD of 1951 from Berkeley. He has shuttled back and forth between the two continents, but has been teaching at the University of Western Australia most of the last eight years. He made one more visit to America which resulted in his starting in on a collaborative book with T. R. Sarbin and D. E. Bailey, Cognitive Theory and Clinical Inference, which is due out by Rinehart in the spring of 1960.

EVERY writer of a book in a broad area of psychology has to make up his mind whether to produce a textbook or a systematic contribution. Guilford is quite forthright about his aim in this book: he is writing a text for upper-division students. This distinguished teacher from the University of Southern California has a topnotch record of achievement in the writing of such books; his *Psychometric Methods* is justly regarded as a masterpiece in exposition and must be considered one of the important landmarks in the development of psychological measurement.

Many teachers will adopt this new book for their course on personality, sight unseen on the author's reputation alone, but quite a few of them will be disappointed with it, for it lacks the balanced and comprehensive coverage required. It seems likely that Guilford's involvement with his own important contributions to the study of personality have contributed to this defect. He claims that he has attempted to present the picture of personality from the one point of view of the statistical and experimental, and much of the subject matter that is conventionally included under the rubric *Personality* he has de-

liberately excluded or dealt with only summarily. Some of his notable omissions are: the development of personality, hereditary factors in personality, personality in relation to culture and social structure, the dynamics of abnormal behavior, the interplay and integration of dynamic aspects (Guilford calls them *hormetic traits*), and the self.

What does this book called *Personality* contain, then? It includes a general introduction on the nature of the concepts of personality and the meaning of traits, a discussion of various methods of assessment and their validity, and an integration of the literature on the dimensions of personality. This treatment clearly brings Guilford's work into the group of personality theories described by Hall and Lindzey as *Factor Theories* and its scope resembles most that of Eysenck's (see the reviewer's letter concerning Hall and Lindzey, *CP*, May 1958, 3, 143f.). It differs, however, from Eysenck's contributions in two important ways: in its preference for many specific rather than a few broad, independent traits and in its avoidance of an attempt to explain traits in terms of some other constructs.

Guilford specifically withholds his apologies for giving his book such a comprehensive title. To him the scope of the book correctly represents *personality* which he defines as an individual's unique pattern of traits. It is an author's privilege to define and handle his matter as he wishes, yet he has some obligation to potential purchasers and adopters of his book to describe its contents according to a usual consensus. Guilford objects to the very appropriate title *Description and Measurement of Personality* on the grounds that this title has been pre-empted, but other apt

ones are available—for example, *Personality, its Structure and Measurement*. In any case the title *Personality* has also been used before.

As a text, the book is not coordinate with the typical upper-division course in personality, but it will fit adequately the minority of such courses as are oriented towards the phenotypical structure of personality, and especially those personality courses that are related to problems of assessment and vocational selection. It will also serve as a useful supplementary text for more orthodox courses.

THE book commences with a down-to-earth treatment of basic definitions and a very competent lining up of some of the perennial issues in the field; unique versus common traits, the nature of intuition, the place of typologies, the hierarchical structure of personality, the dimensional properties of traits. The section on various methods of assessing personality is a valuable critical review which varies in quality from a rather weak treatment of attitude measurement to excellently balanced chapters on somatic measurements (morphological and physiological), inventory tests and general clinical methods. In these sections Guilford's long experience as a constructor of factor-based tests, and as an aviation psychologist working on problems of selection, lend a refreshing sense of reality to the discussions. The book concludes with an extremely useful pulling together of the various superficial personality dimensions that have been identified in the experimental literature.

The writing is at a level of psychological and statistical sophistication that is appropriate for the average upper-division student. The introduction of necessary statistical concepts, like analysis of variance, is handled with a delicacy that is most unusual in a writer who is a heavyweight in this field, and there is an eight-page appendix that appears adequately to serve the purpose of reminding the student of what he learnt in his introductory course in statistics and of introducing him to factor theory.

The student who uses the book will emerge from his course with a healthy

Significant Titles from THE CENTURY PSYCHOLOGY SERIES

Toward Understanding Human Personalities

By ROBERT LEEPER and PETER MADISON. Based on the findings in psychotherapy, anthropology, and experimental psychology, this book emphasizes the psychology of normal persons rather than abnormal psychology. Following an introductory discussion which reveals the limitations of our everyday concepts about personality, the main psychological aspects and problems of personality are discussed in more generalized terms concerning motivation, strategies of living, and learning. Detailed case histories illustrate the text. 439 pp., \$5.50.

Developmental Psychology, 3rd Edition

By FLORENCE L. GOODENOUGH and LEONA E. TYLER. As in previous editions, this text presents the fundamental concepts of psychology in a simple and stimulating account of human growth. Greater emphasis is placed on personality development, including the work based on psychoanalytical approaches to child study, and on developmental tasks as the distinguishing features of successive life stages. Particular attention is now given to the ideas of Piaget and Erickson. 552 pp., illus., \$6.00.

Educational Psychology

By GEORGE G. THOMPSON, ERIC F. GARDNER, and FRANCIS J. DI VESTA. A comprehensive overview of the concepts and principles of growth, learning, and adjustment, this text correlates both recent theories and traditional principles with numerous applications to classroom usage. It presents detailed treatments of personality development and group dynamics, while covering adequately the conventional topics of educational psychology. In addition, it provides a practical approach to pupil evaluation that considers methods of formal testing. 535 pp., illus., \$6.00.

Verbal Behavior

By B. F. SKINNER. Representing Dr. Skinner's definitive work on the subject, this well-known book follows the pattern of a functional analysis of behavior, closely tied in with earlier and current experimental analyses of behavior in general. It discusses the conditions responsible for the acquisition and maintenance of various kinds of verbal behavior and the effects of multiple variables in selecting particular forms of response and in probing verbal behavior clinically. 478 pp., \$5.50.

Psychological Research

By BENTON J. UNDERWOOD. This text for graduate courses and seminars offers a critical evaluation of contemporary research practices. Discussions on stimulus analysis and response measurements and on specification and definition of behavioral phenomena are followed by an analysis of common errors in the design and interpretation of experiments and by a section on the nature of explanatory concepts and how they enter into explanatory attempts. 298 pp., \$4.50.

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J. P. GUILFORD

respect for rigor in the study of personality, but a somewhat biased perspective on the position enjoyed by multiple factor techniques in the scientific study of personality. In this connection the tyro may well be misled by the hoary semantic fallacy "that many obtained factors should be taken seriously as representing psychological reality." Guilford confesses that factor analysis has its deficiencies for providing "a general, rational basis for the description of personality," but he does not discuss these limitations with the reader in any detail.

THERE are several unexpectedly imprecise expressions and arguments in the book that may leave a thoughtful reader saying to himself, "Just what does he mean by that?" Here are a few examples:

Page 90: "From the psychoanalysts we have the personality types that are attributed to Freud—his oral, anal and phallic types." Why "attributed?"

Page 95: Having diagrammatically illustrated two tests as overlapping ellipses, Guilford states that "the shape is not very important." Actually the shape is of *no* apparent importance at all.

Page 493, Table A1: A score of 1 is equivalent to L, a score of 2 to W, and a score of 3 to 2L + W. This is difficult

to figure out, especially as no reference is even made to this table for another five pages.

One confusion in definition is worth mentioning. In defining *scalability* he mixes the usually accepted meaning of unidimensionality with that of continuity. A trait is described as scalable when "different individuals have different degrees of it," and unscalable when it is either present or absent as, for example, having a phobia or a tic. In practice it is possible to describe the presence or absence of any trait in the process of measurement, for example, extraverted or not extraverted, and it is also possible to describe degrees of having a phobia or a tic. The scalability of a trait surely could not rest on this measurement fiat.

Certain deficiencies in technical organization create unnecessary obstacles to easy reading: meaningless headings such as *Features to be noted*, the presentation of logically subordinate headings as peers, and the use of small black headings to transcend larger gray ones.

So much for this book as a text. The reviewer considers that it will also find extensive use as a handbook for psychologists who are working with personality concepts in research or assessment. It offers the assessor a vast catalogue of empirically established dimensions—physical, intellectual, temperamental, motivational and pathological—from which to choose in order to describe personality in a coherent fashion, and it thus provides an alternative to the work of Eysenck and Cattell. Eysenck's factors are, as Guilford points out, too broad for practical value in assessment, although breadth has its virtues for explanation in personality theory. Cattell's

factors can less easily be dismissed on these grounds and Guilford's objections to them is that confusion can arise from his "use of experimental variables from different levels in the hierarchy," most of them at a higher level of generality than Guilford's preferred level. Nevertheless Guilford makes use of Cattell's findings where he considers them to be applicable.

This extremely atomistic approach to personality has further advantages. It avoids such irksome issues as the correlation between the factors and the question of surface versus source traits. The approach serves as a launching pad for systematic probes into personality, but in the endeavor to use empirical methods to understand personality—Guilford has left off very close to the point where Cattell started, that is to say, at the definition of surface traits. Perhaps he has done so advisedly, but, if so, he seems to have had mixed feelings about it. Guilford pays tribute to the "great advantages to finding dimensions located in the personality sphere that are independent or nearly independent." To apply this principle would lead him to a few high-level traits rather than many low-level ones. He also argues that in the trait hierarchy "determination is downward" and one might therefore expect him to pay more respect to the study of higher-level traits (or "types"). He seems finally to rest his case on the fact that lower-level traits are more valuable to the vocational psychologist. In taking this point of view he has produced a valuable introduction to the study of the structure and measurement of personality, but his contribution to the understanding of personality is necessarily limited thereby.



To spend too much time in studies is sloth; to use them too much for ornament is affectation; to make judgment wholly by their rules is the humor of a scholar. They perfect nature, and are perfected by experience, for natural abilities are like natural plants, that need pruning by study; and studies themselves do give forth directions too much at large, except they be bounded in and by experience. Crafty men condemn studies, simple men admire them, and wise men use them for they teach not their own use; but that is a wisdom without them, and above them, won by observation.

—FRANCIS BACON



Coming in July . . .

HANDBOOK OF RESEARCH METHODS IN CHILD DEVELOPMENT

Edited by PAUL HENRY MUSSEN, *University of California, Berkeley.* Here at last is a book which provides broad coverage of research methodology in the study of child development. Not only does it offer highly specific accounts of particular research methods and clinical techniques, it also deals with the general methodological issues which the field involves. The editor has organized the work in the following way: Part I discusses general research design and the use of observation and experimental methods in the field of child development; Part II deals with the study of biological growth and development; Part III is concerned with the study of cognitive processes (perception, learning, language, etc.); and the final section presents the major research tools used in the study of personality development and social behavior.

Each chapter of the book was written by an outstanding research worker or teacher in the field, including such prominent figures as R. R. Sears, Eleanor Maccoby, Boyd McCandless, Roger Barker, Milton Senn, Dale Harris, and Harold Stevenson. The research wisdom and "know-how" provided by these and the other contributors can readily be applied in many areas of study other than child development. 1960. *In press.*

PSYCHOLOGICAL SCALING

Theory and Applications

Edited by HAROLD GULLIKSEN and SAMUEL MESSICK, *Educational Testing Service.* Based on papers presented at a conference on psychological scaling held at Princeton, New Jersey, in May, 1958, this book discusses the basic theory of measurement and shows its various applications to problems in attitude measurement, utility measurement, measurement of intensity or quality of sensation, and measurement of mental abilities. 1960. *In press.*

GUIDANCE OF THE YOUNG CHILD

By LOUISE M. LANGFORD, *Kansas State University.* This book offers practical guidance methods for those who supervise the preschool child in individual and group activities. The techniques are based on research studies of growth patterns, specific patterns of growth and development, and behavioral responses at various maturational levels. Many case histories are given to illustrate actual situations. 1960. 349 pages. \$6.25

WORD AND OBJECT

By WILLARD VAN ORMAN QUINE, *Harvard University.* In this work a well-known philosopher and logician analyzes the basis of meaning and communication—the relation between linguistic forms and the verbal and non-verbal stimuli with which these forms can be correlated. Using the insights of natural linguistics and psychology, Professor Quine examines the difficulties involved in translation, brings to light the anomalies and conflicts implicit in our language's referential apparatus, and develops remedies for these along the lines of modern logic. 1960. 294 pages. Prob. \$5.50.

CHANGING PATTERNS OF INDUSTRIAL RELATIONS

By ARTHUR M. ROSS and PAUL T. HARTMAN, *University of California, Berkeley.* One of a series from the research program of the Institute of Relations, University of California. 1960. Approx. 230 pages. Prob. \$6.50.

MODERN PROBABILITY THEORY AND ITS APPLICATIONS

By EMANUEL PARZEN, *Stanford University.* One of the Wiley Publications in Statistics, Walter A. Shewhart and S. S. Wilks, Editors. 1960. 464 pages. \$10.75

Send for examination copies.

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440 Park Avenue South, New York 16, N.Y.

The Anatomy of Interpersonal Power

Dorwin Cartwright (Ed.)

Studies in Social Power. Ann Arbor: Research Center for Group Dynamics, Institute for Social Research, University of Michigan, 1959. Pp. x + 225.

Reviewed by EDWARD E. JONES

Dr. Jones is Associate Professor of Psychology at Duke University, where he has been for the seven years since his doctorate in the Harvard Department of Social Relations. He is a social psychologist, touched with the brush of Jerome Bruner, and now thoroughly concerned with the social determinants of interpersonal perception, interactive behavior, and the role of attitudes in learning. He edits the Journal of Personality.

SOCIAL POWER may be roughly defined as the potential that one person has for influencing a particular other in some particular respect. The authors of the present volume intend to convince us that we need such a concept to understand many of the phenomena of interpersonal behavior. They pursue this goal by the dual strategy of presenting a variety of empirical studies in which the concept of power is employed, and by making a serious attempt at conceptual analysis along field-theoretical lines. The result is a rather curious but on the whole useful and impressive collection.

This volume is an institutional venture in that each chapter was authored by a staff member of the Michigan Research Center for Group Dynamics. In actual content, the book comprises an orienting chapter (Cartwright's 1953 SPSSI address), portions of five doctoral dissertations, two other empirical reports, and three concluding theoretical chapters. Though none of this material has been published elsewhere, four of the dissertations were completed in 1953, the fifth in 1955. This chronology presumably explains why most of the distinctions drawn in the theoretical

chapters are not reflected in the design and interpretation of the empirical studies.

Of the seven empirical studies reported, four are laboratory experiments, two are surveys, and one is a field study. The variety of procedures, settings, and samples clearly adds to the demonstrational value of these studies. They point up the ubiquity of power relations whenever and wherever persons interact and indicate the range of methods appropriate for the study of power. The treatment shows a healthy concern with adapting one's measures and procedures to the problem at hand, and it is encouraging to note that attempts have been made to manipulate or study power relations within a meaningful institutional context—in two cases actual supervisors engage in experimentally controlled behavior.

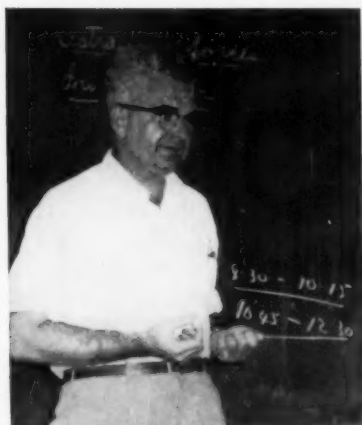
For various reasons which cannot be detailed here, each study is, however, open to major or minor criticisms as a finished product of deduction, evidence, and interpretation. In some the reasoning is fuzzy or unconvincing; in others differential power is a constant whose effects are impossible to assess; in still others the variables manipulated are so crude that alternative explanations must be dealt with by a circumstantial appeal to post-experimental questionnaire data.

In combination, the empirical studies do indeed point up the variety of human relationships in which power differentials appear. We learn, moreover, that these power differentials may be accurately perceived by children (at least if they are 'well adjusted'), that subjects can adjust their perceptions of

their own relative power to accord with the ease of persuading another person, that peer-group support helps to create oppositional power in confronting an authority, and that those leaders most accepted by their followers tend to be most effective in changing group productivity. These findings are intrinsically interesting and, for the most part, nicely confirm the hypotheses prompting the individual studies. The hypotheses themselves, however, derive from a scattered array of stated and implicit assumptions which bear little systematic relation to each other. The reasoning in one study is generally compatible with the reasoning in others, but there are few clues regarding the shape and propositional structure of an integrative theory of social power. It is by no means always clear how one gets from one of these studies to another or whether they share anything beyond a concern with power relations.

THE theoretical chapters are excellent in their own right but do not provide connective tissue for the empirical chapters preceding them. In a useful analysis of the bases of social power, French and Raven distinguish between reward power, coercive power, legitimate power, referent power, and expert power. In the main this typology seems valuable in that it suggests some of the consequences of different sorts of power for the influence process. Cartwright attempts a more comprehensive conceptual analysis of power by relating it to its Lewinian ancestor, force. Since these essays complement each other, it is reasonable to discuss their contribution together.

It is clear that a great deal of thought and discussion went into the theoretical chapters. There are few points of inconsistency between the French-Raven and Cartwright discussions of power, and these involve minor shadings of interpretation. The specification of power as the ability of a particular *O* to perform a particular act *a* which sets up a force on *P* is maintained throughout. The fact that this field of force which *O* imposes on *P* must be defined jointly in terms of the strength of *O*'s act and its relation to a "motive base" in *P* stresses the relational notion of the con-



DORWIN CARTWRIGHT

cept of power. The concept is even defined in terms of the difference between the positive force and the resistance evoked by the particular act of *O*. But the advantages of such a relational definition are obscured by two important considerations, both stemming from the fact that the theory is couched largely in terms of unobservables. First of all, the conditions under which the potential to exert influence becomes actualized are not dealt with in any systematic fashion. One kind of problem which is not discussed, for example, is the extent to which the use of available power may be inhibited for various reasons—such as a fear of diluting its strength, disrupting the relationship, etc. Secondly, we cannot predict the behavior of *P* even if we have full knowledge of the force field imposed on him by *O*. *P* is being constantly affected by other force fields which can create varying degrees of opposition to the influence exerted by *O*. There is, however, no systematic treatment of the sources of this opposition to the exerted influence. Thus it is not clear how we get from the act of the power agent to the behavior of the target of his influence. (It is interesting to note that the empirical studies avoid this difficulty by essentially equating power with the ability for effective influence. Only future research can point up the consequences of a distinction between power and the ability to control.)

A critical evaluation of Cartwright's conceptual statement would necessarily

involve an evaluation of Lewinian field theory itself. It was Cartwright's hope that by relating the concept of power to a more general system of behavior theory, "hypotheses can readily be generated concerning the empirical relations of power to motivation, cognition, modification of behavior, and other psychological attributes." Unfortunately the field-theoretical parent itself has long had to live with difficult problems of conceptual coordination and operational specification. The abstract, formal quality of Lewinian language seems nicely designed to handle many of the nuances of distinction which must be considered in a comprehensive theory of power. Cartwright's emphasis, however, is more on the vocabulary of power than on its syntax. Our ability to describe interpersonal events is advanced by his treatment, but the capacity to predict these events seems to depend on considerations not yet embraced by the theory.

In conclusion, let it be said that this is a book which should be seriously examined by students of social interaction. The modest, exploratory aims of the book are clearly stated and honorably pursued. Those who are in search of a theoretical breakthrough or a mine of rich empirical ore will probably be disappointed, but there is much here to arouse one's interest in the use of 'power' as a conceptual wedge. At least the present volume helps us to understand better what it is that we need to know.



Most people are determined to hold the line against animals. Grant them the ability to make linguistic reference and they will be putting in a claim for minds and souls. The whole phyletic scale will come trooping into Heaven demanding immortality for every tadpole and hippopotamus. Better to be firm now and make it clear that man alone can use language to make reference.

—ROGER BROWN



Of Arms and the Man

J. N. Spuhler (Ed.)

The Evolution of Man's Capacity for Culture. (Six essays by J. N. Spuhler, Ralph W. Gerard, S. L. Washburn, Charles F. Hockett, Harry F. Harlow, and Marshall D. Sahlins, with a summary by Leslie A. White.) Detroit: Wayne University Press, 1959. Pp. 79. \$3.50.

Reviewed by WILLIAM R. THOMPSON

who is Associate Professor of Psychology at Wesleyan University, although at present at the University of Queensland in Australia, where on a Guggenheim Fellowship he is putting together his research on the influence of prenatal maternal stress on the behavior of offspring. He has been concerned with the problem of heredity and environment ever since he worked in D. O. Hebb's laboratory at McGill University. With J. L. Fuller he will soon have out, via Wiley, a book called Behavior Genetics.

MAN has culture. Human culture is so complex and manifold that it is very difficult to extract its main dimensions and link them to some basic components of man's nature. For the same reason, it is even more of a task to discover its evolutionary origins in prehistory. In Darwin's centennial year, the six essays making up this concise volume attempt to bring new evidence to bear on the problems: What is culture? Where and how did it come about?

The authors involved are all experts in different fields. J. N. Spuhler is a geneticist, R. W. Gerard a neurophysiologist, S. L. Washburn a physical anthropologist, M. D. Sahlins a cultural anthropologist, C. F. Hockett a linguist, and H. F. Harlow a psychologist. L. A. White, who supplies the book with its own review at the end, is also a cultural anthropologist.

This variety of authorship makes the book's unifying theme difficult to un-

cover. Perhaps it can best be elucidated by reference to Richard Kinney's design on the dust jacket. Here are contrasted man and ape. The ape, silhouetted in the background, squats with one hand outstretched in front of him, the other partly supporting him. Man, a detailed Athenian figure, stands erect, one hand also raised, with thumb and index finger crooked in opposition. A part of his hand and leg overlap the figure of the ape. This simple drawing symbolically depicts the continuities and discontinuities between lower primates and man, and the somatic paths linking them.

Thus Spuhler and Washburn bring together in their respective essays a variety of paleontological and archeological evidence to indicate that erect stance or bipedalism, having freed the simian hand from its locomotor function, anatomically molded it for manipulation and tool-using. Selection, acting on these master-adaptations, then allowed the sudden (in paleontological terms) emergence of a variety of new structural and functional characters that made culture possible. Washburn says, "In a very real sense, tools created *Homo sapiens*." He and Spuhler neatly describe how these changes evolved from lower primates through Proconsul in the Miocene period up to early man in the late Pleistocene.

If one use of hands is tool-using, another is simply clinging. Harlow, in a rather novel twist, presents some of his data on 'monkey love' to document the point that this function of arms and hands is crucial to the establishment of those affiliative tendencies that must underlie any social grouping.

What structural alterations may have been produced by tool-using? Presumably, as Gerard suggests, the most basic must have been changes in the central nervous system, not only in absolute and relative brain size, but also in degrees of centralization, and in the electrophysiological properties of its neuronal units. Dealing with the functional side, Harlow, Hockett, and White describe the behavioral capacities of learning, language, and symboling that appear to underlie human culture. Their close analyses reveal both continuities and discontinuities between lower ani-

mals and man for different aspects of these two broad traits.

WHAT then, in the end, are we to say about human and animal societies? Do monkey and ape groups in any sense have culture as we know it, or are they categorically different? Sahlins' answer to this question emphasizes mainly the differences rather than the likenesses. Although continuities exist—territoriality, and sex and age groupings are examples—discontinuities are more prominent. Sex and aggression, the most basic drives governing primate groups, are replaced even in the most primitive of human societies by economic motives involving cooperation and sharing.

Evaluation of such a book is not easy, precisely because of its terseness and the diversity of its theory and data. I think one could argue about a number of specific points. For example, the important tool-using thesis of Washburn is highly speculative and based on rather scanty evidence, as he himself admits. Furthermore, his identification of culture with tool usage needs qualification. Many species of birds and insects also use tools. Again, the opposition made by Sahlins between the savage competitiveness of ape groups and the peaceful cooperativeness of human societies seems overdrawn. Certainly a Freudian, at least, would consider it naive to regard economic activities, incest taboos, and the like as 'replacing' motives of sex and aggression. In its general thrust, however, the book has importance on at least two counts. In the first place, it clarifies the often misunderstood role of comparative psychology by showing that this discipline can not only give breadth to our definition of behavior by its inclusion of lower animals, but can also illuminate the uniquenesses of human beings by pointing up discontinuities and contrasts between man and lower animals. To understand *A*, we must also understand not-*A*. Secondly, the discussions serve as a reminder that differences of opinion or theory with regard to some general issue need not always stand in opposition, but rather can complement one another, each adding in its own way to the explication of the central problem. There is strength in diversity, as well as in unity.

Not Your Theory but How You Use It

Fritz Redl and William W. Wattenberg

Mental Hygiene in Teaching. (2nd ed.) New York: Harcourt, Brace, 1959. Pp. xiv + 562. \$5.50.

Reviewed by JOSEPH MASLING

who is Director of the Training Program in Clinical Psychology at Syracuse University. Ten years ago he actually worked with Fritz Redl but CP thinks him fully qualified now to review Redl's book. He also has the traces of influence by Ronald Lippitt, and presently by George Kelly. Just now he is working with George Stern on the pedagogical significance of unconscious motives in teaching.

THE first edition of *Mental Hygiene in Teaching*, while well written and insightful, suffered seriously from inadequacy of documentation. The second edition has remedied this defect, at the same time retaining the unique flavor which comes from the collaboration of a culturally oriented psychoanalyst (Redl) with a dynamically oriented sociologist (Wattenberg). Each author has worked with delinquent boys as well as with teachers, so that the book bears unmistakable evidence of being knowledgeable about its subject matter, a pleasant, if unusual, quality for texts in this general area.

The book is written for the classroom teacher. The language is usually free from psychological jargon, the case histories will sound familiar to most teachers, and theory is never allowed to interfere with the presentation of the more common issues that teachers must deal with. Such problem situations as the teacher-parent relationship, the roles of the substitute teacher, the supervising teacher, the principal and the student teacher, the frustrations of teaching, all are given full discussion. Many texts treat the question of discipline as though it were contaminating, mentioning its importance but avoiding direct contact with

it at all cost. This book faces squarely the issue of discipline in terms the teacher can understand. The index lists 40 references to punishment and 18 references to discipline; a chapter is devoted to *Influence Techniques* and there is a separate section on teachers' problems. *How Good or Bad is it to Get Angry?* One can hardly expect more explicit discussion than that.

Not all of the book is devoted to such mundane considerations nor does all the volume resemble a do-it-yourself guide to mental hygiene. The treatment of the importance of the group is excellent, and that, of course, is hardly a surprise to anyone familiar with Redl's previous work. The chapter on defense mechanisms is substantial, lucid, and well organized; it is by far the best discussion of defense mechanisms in an undergraduate text that I have seen. Of particular value is the authors' awareness of the existence of forces external to teacher and child; such variables as social class, prejudice, and societal pres-

ures are included in discussions of the classroom situation.

If the first edition erred in the direction of making authoritative statements without documentation, the present edition provides all the documentation one could ask for and then some. What other book on mental hygiene can boast of references to studies published in the *Glasgow Medical Journal*, *University High School Journal*, *Butler County Mental Hygiene Association*, *Delaware State Medical Journal*, *Journal of School Health*, and the *English Journal*? Unpublished doctoral dissertations are referred to by the score, as are the more usual experimental studies. Unfortunately, the usefulness of this vast amount of documentation is severely limited by the failure to provide a compilation of references at the end of the book.

Despite the wealth of references there still remain systematic omissions of important studies. Although the book has a sociological flavor, its basic ingredient is psychoanalytic, and one looks in vain

for references to studies which are critical of psychoanalytic hypotheses. For example, in the matter of the influence of early infant-rearing techniques: an unpublished dissertation is cited to show that coercive toilet training leads to negativism and immaturity at age four, but the studies of Orlansky, Sewell and Mussen, Faber and Sutton, Lindesmith and Strauss, and Peterson and Dano, all which fail to support the importance of specific methods of child-rearing, are nowhere mentioned. This point of view may be said to be reformed Freudianism, but it is unquestionably Freudian; heretics would have a difficult time gaining recognition.

Redl has remarked that, "It ain't your pathology, it's the way you use it." That theory here is used to present important issues to teachers in the language, concepts, and form they can understand. All told, the slant is Freudian, the area of consideration includes forces outside the teacher and child, and the writing is done with knowledge, taste, and sophistication.

An Important Announcement from McGraw-Hill Book Company

McGraw-Hill is proud to announce the publication in May, 1960, of

BARGAINING AND GROUP DECISION MAKING

by SIDNEY SIEGEL and LAWRENCE E. FOURAKER,
both of the Pennsylvania State University.

The authors received for this work the first Monograph Prize Award in the Social Sciences from the American Academy of Arts and Sciences. In their research, the authors have brought together their fields of study, psychology and economics.

The research reported in the monograph was conducted under research grants from the National Science Foundation and the Ford Foundation. Facilities of the departments of psychology and economics at Pennsylvania State University were used in the experimentation.

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The Proper Domain of Psychotherapy

Jules H. Masserman and J. L. Moreno (Eds.)

Progress in Psychotherapy. Vol. IV: *Social Psychotherapy*. New York: Grune & Stratton, 1959. Pp. xii + 361. \$8.75.

Reviewed by DAVID P. AUSUBEL

who is Professor of Education in the University of Illinois' Bureau of Educational Research. Actually he is a psychiatrist and experimental psychologist, with some field experience in anthropology thrown in. He is primarily concerned with the problems of personality and child development. He is the author of *Ego Development and the Personality Disorders* (Grune and Stratton, 1952), *Theory and Problems of Adolescent Development* (Grune and Stratton, 1954), *Theory and Problems of Child Development* (Grune and Stratton, 1958; CP, Jan. 1959, 4, 7f.), and *Drug Addiction* (Random House, 1958; CP, June 1959, 4, 133f.).

THIS fourth volume in an annual series devoted to progress in psychotherapy closely follows the pattern established by its predecessors with respect to scope, approach, level, and organization. The primary purpose of the series is to bring practitioners and other persons interested in the field up to date on current professional problems, ideological trends, and innovations in technique. Volume IV, like the earlier volumes in the series, enjoys a fair measure of success in realizing this objective: it provides a provocative and yet relatively painless way of keeping abreast of new developments. By the same token, however, although it contains many stimulating discussions of important theoretical, practical, and professional problems, the book lacks the comprehensive and systematic coverage and the pulling together and interpretation of scattered findings that one ex-

pects in a survey of recent progress in a relatively circumscribed area of psychological knowledge. At best, only passing reference is made to the research literature.

The theme and subtitle of the 1959 volume is *Social Psychotherapy*. Much of the material, therefore, is quite understandably concerned with such topics as group psychotherapy, psychodrama, milieu therapy, and the 'therapeutic community.' Such related theoretical issues as the influence of various economic and sociocultural factors on the incidence and symptomatology of mental disorders are also clearly relevant to the general theme. Somewhat more difficult to appreciate, however, is the relevance of articles dealing with such topics as the teaching and private practice of individual psychotherapy, the dynamics of control and 'transference cure' in psychoanalytic therapy, and the psychopharmacology and use of drugs in psychotherapy. The inclusion of these latter articles makes for much unnecessary and disconcerting unrelatedness of content.

AN issue that recurs in several different contexts has to do with the proper domain of psychotherapy. The expansionist trend, represented by Moreno, envisions the society of the future as a 'therapocracy,' that is to say, a therapeutic world order in which politics, science, religion, and economics, as well as physical and mental hygiene, are all under the control of a group of presumably omniscient therapists. Relating this dictum to the methodology of social research, Moreno insists that anthropology can never be more than a "science fiction story," dealing with unreal "clusters of variables" torn out of their social context, until it focuses on the production and analysis of therapeutic change in the community under investigation.

Masserman, on the other hand, cautions against the uncritical 'overselling' and application of psychological concepts to other fields of human knowledge and endeavor. When misapplied in this way, he maintains, "psychoanalytic formulations may be regarded as no more scientific, say, than were phrenology or mesmerism, both of which

in their day could also claim plausible-sounding, internally consistent . . . theoretical systems that either ignored or encompassed all other sciences." And in a similar vein, Wilder and Lauterbach suggest that psychotherapists seriously study socioeconomic systems so that they can appraise their own social roles and limitations more realistically and thus "beware of attempting to solve all of the problems of the nation and the world by psychotherapy."

THE issue of psychotherapeutic effectiveness is a related problem which receives considerable attention throughout the volume. Several contributors acknowledge the absence of any convincing objective or scientifically acceptable evidence that psychotherapy is effective. They then go on to assert that the presence or absence of such evidence really does not matter as long as therapist and patient both *know* that growth and increased satisfaction have occurred. Furthermore, they insist that what happens in psychotherapy is a private experience "not accessible to verification."

No branch of the healing art, in this reviewer's opinion, can ever hope to achieve scientific status if it seeks exemption from its rightful obligation of proving that patients undergoing therapy improve reliably more frequently than comparable patients left untreated. Since most human ailments tend to improve spontaneously, intuitive certainty, client satisfaction, and frequent demonstrable success do not constitute adequate proof of therapeutic efficacy. The history of medicine abounds in instances of quacks and charlatans who not only received glowing testimonials from satisfied patients, but who also believed sincerely in the value of their worthless nostrums.

If gross studies of psychotherapeutic effectiveness tend to yield disappointing findings, the next logical step might be to ascertain through more refined research techniques whether psychotherapy as a general method actually holds little promise for ameliorating behavior disorders, or whether its seeming ineffectualness is less real than artifactual. Thus, the apparent failure of psychotherapy in many instances may simply reflect the use of inappropriate meth-

ods of measuring or evaluating treatment outcomes. In other instances negative results may possibly reflect the limited usefulness of certain specific practices or their indiscriminate application to basically unsuitable cases. Finally, research may conceivably indicate that, although psychotherapeutic tools are only effective in the hands of individuals with particular personality traits, these traits are not especially conspicuous among present-day practitioners of the art.

Trainers in Human Relations

Matthew B. Miles

Learning to Work in Groups: A Program Guide for Educational Leaders. New York: Bureau of Publications, Teachers College, Columbia University, 1959. Pp. xvi + 285. \$5.00.

Reviewed by HOWARD BAUMGARTEL

who is Associate Professor of Human Relations, Psychology, and Business Administration at the University of Kansas and chairman of its Department of Human Relations. He is busy with the development of courses in human relations, with the assessment of personality changes associated with training in human relations, and the study of social-psychological factors in industrial settings. For CP he reviewed Nadel's Theory of Social Structure (*Free Press*, 1957; CP, Nov. 1957, 2, 297f.).

YEARs ago F. J. Roethlisberger pleaded for the development of social inventions to help us deal with the pressing problems of human collaboration in our conflictful and anomic industrial society. This book by Matthew B. Miles, Associate Professor of Education at Columbia Teachers College and a Research Associate in the Horace Mann-Lincoln Institute of School Experimentation, gives ample evidence of

Outstanding books in psychology . . .

PERSONALITY

Development and Assessment

Charles M. Harsh, formerly University of Nebraska; and H. G. Schrickel, Massachusetts State Teachers College, North Adams

This popular textbook presents the basic data, theory, and methods necessary for understanding the psychology of personality. Treating the personality as a pattern of system functions, the book describes the emergence and development of the self from infancy to old age. Emphasis is placed on such concepts as sensitization, arousal, and response elicitation. Book reviews the-

ories of personality and methods of assessment; reflects the current emphasis on self theory, Q-sorts, and better design of factorial and experimental studies. Summarizes recent research findings, stresses the challenging demands that must be met by any comprehensive theory of personality. 2nd Ed., 1959. Illus., 536 pp. \$6.75

INTERPERSONAL DIAGNOSIS of PERSONALITY

A Functional Theory and Methodology for Personality Evaluation

Timothy Leary, Kaiser Foundation Hospital

A pioneering book which describes techniques for measuring interpersonal expressions at different personality levels, and an empirical method for applying the techniques in diagnosis. New theories about interpersonal behaviour, the meaning of fantasy, the social language of symptoms, and the nature and meaning of conflict are set forth, together with research results

which support these new conceptions. In addition to describing and validating the process of interpersonal diagnosis in the psychiatric clinic, book outlines the application of these theories and methods in the psychiatric hospital, in psychosomatic medicine, in industrial management, and in the group therapy situation. 1957. 120 illus., tables; 518 pp. \$12

SOCIAL PSYCHOLOGY

An Introduction to the Study of Human Relations

S. Stansfeld Sargent, Clinical Psychologist, Veterans Administration, Phoenix; and Robert C. Williamson, Los Angeles City College

Offering an integrative approach to the science of social behavior, this widely used textbook incorporates basic principles and data from the whole range of the social sciences. To provide a broad conceptual framework, the authors have adopted a field approach which includes personality variables, situational factors, and persons' perceptions

and interpretations of social situations. The book first presents the fundamental principles and theories of the field; then shows applications to the various areas of human behavior. "The best I have seen for an introductory course."—Rev. F. C. Keeler S. J., Creighton University. Instructor's Manual available. 2nd Ed., 1958. 31 illus., 649 pp. \$6.50

The CARICATURE of LOVE

A Discussion of Social, Psychiatric, and Literary Manifestations of Pathologic Sexuality

Hervey Cleckley, M.D., Medical College of Georgia

Analyzing instances of the perversion of erotic experience in literature and life, this outstanding book also examines concepts popular today in psychology and psychiatry which the author considers unproved and harmful when used by laymen to support the belief that sexual disorder—particularly homosexuality—is natural. Illustrated with case histories of Dr. Cleck-

ley's patients and pertinent literary references. "Searching and illuminating . . . a straightforward antidote for some of the most serious current superficialities about sex and love."—William S. Taylor in *Contemporary Psychology*. A volume in A Psychology Series edited by J. McV. Hunt. 1957. 319 pp. \$7

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the inventiveness of one group of professionals in human relations. As such this book, along with Lippitt, Watson, and Westie's *Dynamics of Planned Change* and the forthcoming book on *T-Group Theory*, provides a major statement of the point of view and techniques of the loose confederation of educational and social psychologists associated with the National Training Laboratory in Group Development. Professor Miles is, of course, a member of the National Board of the NTL.

While the book is written for trainers and people otherwise interested in "improving group work in schools," most of the material is directly transferable to leadership and small-group training problems in a variety of institutional settings. People in industry and schools of business working with training can well take note of the ingenious training designs of Miles and his associates. This book will, however, as Miles states, have little meaning to a person who is not interested in improving the groups of which he is a part.

THE text deals explicitly with interpersonal and small-group factors in the work setting. Personality variables, organizational and community analysis are not at its focus. It is based on the familiar 'Bethel' ideas about groups, leadership, and training. Learning to be effective in groups is a matter of acquiring sensitivity, diagnostic skills, and behavioral skills. Leadership is viewed from the functional approach, not as personality trait or in relation to formal authority. Leadership and membership skills are held to be learnable. Training in human relations deals only with the social self and social behavior, leaving the deeper dynamics of the personality alone. Training or learning takes place best in an environment where people are important, where new behaviors can be experimented with, where feelings are of paramount importance, where things are not taken personally, where learning is by doing and process analysis, where attention is focused on the here and now, and where group planning is the norm for all activities. How much these characteristics remind us of the atmosphere of the psychotherapeutic relationship and how little they re-

semble the human environment in most people's worlds of work!

The book contains invaluable how-to-do-it descriptions of program plans as well as dozens of inventive training exercises, all conveniently indexed by problem (p. 100) and by type of exercise (p. 175). Both the text and the excellent bibliography cover almost every different type of training device and technique developed by the National Training Laboratory and similar groups from 'alter egos' to the use of tape recordings in training. The importance of action research on the effectiveness of training is stressed, but little evidence from research is referred to. The prerequisites for being a trainer are discussed and this reviewer was shocked to see the "formal and practical knowledge of groups" ranked as seventh of the nine essential characteristics.

Professor Miles knows what he is doing and what he is not doing. The book is filled with trainers' wisdom based on years of clinical practice and 'group thinking.' Miles' problems are those of the field of training research, and consultation in respect of human relations. Are we as sure as we should be of the theoretical base and research for our training objectives and methods? Can we talk about and manipulate the social self apart from the dynamic self? Are we clear about our value premises in the triadic relationship between trainees' desires, clients' organizational goals, and our own impulses to reform or change people? (How are we different from the Chinese communist 'trainers' in the Korean POW camps?) Can we not become obsessed with group dynamics and feelings to the neglect of the task and substantive problems of groups? Why do people come for training anyway—to become 'more effective group members' or to overcome loneliness and a sense of personal isolation? How do we account for the rise of human relations as a social movement? Why do people who live and work in the areas of the deep, static social conflicts—of labor relations, race relations, economic and international competition—sometimes view us as a bunch of Boy Scouts?

Social Formulas

Georg Karlsson

Social Mechanisms: Studies in Sociological Theory. Glencoe, Ill.: Free Press, 1958. Pp. 156. \$5.00.

Reviewed by DONALD T. CAMPBELL

who is Professor of Psychology at Northwestern University—has been there since 1953—and whose particular Fach is social psychology, especially the measurement of social attitudes and the comparative psychology of knowledge.

THIS inventory of mathematical theories is primarily addressed to the sociologist, although drawing heavily upon the works of psychologists and economists. The author is a member of the Sociology faculty of the University of Uppsala and appears well grounded for the type of review he essays.

About one third of this brief book (distributed throughout) is devoted to reviewing some 17 mathematical theories to be found in the literature. These cover social diffusion, sociometric choice, labor mobility, migration, communication, status distribution, and the like. Another third of the book is devoted to illustrative empirical studies in the same areas, unconnected to formal theory but introduced because of judged relevance to the more adequate theories of the future. The remaining third is devoted to the author's own suggestions of alternative mathematical formulations.

The most valuable portion of the book is its review of previous mathematical theories of social process. Karlsson broadens our usual perspectives by including valuable items from the Scandinavian literature, from economics, and from the mathematical biologists (Rashevsky, Landau, Rapoport, Shimbel) as well as the ones (e.g., Simon and Guetzkow) with which we tend to be more familiar. Its usefulness as an advanced text in this area is reduced by the brevity of each treatment, and by an omission of all details of the confrontation of formal theory with data. Although he believes that this confrontation is of greatest importance, and although a

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good share of the models reviewed provide such, he gives no details on this matter, other than such cryptic statements as "gave bad fits," "shows good agreement with experimental data," etc. In particular, this reviewer missed the full reporting of those few instances (e.g., Rapoport on spread of information, cited on pp. 26f.) in which there has been a multistage iteration between formulation and data, resulting in a final formula well disciplined by data and making possible independent confirmation of crucial parameters. The space devoted to the illustrative nontheoretical empirical studies could have been better spent in this way, particularly since Karlsson does not use these latter data to evaluate the particular parameters and functions of the formal theories.

KARLSSON'S own theoretical formulations have in no instance been disciplined by data and, although mathematical in notation, tend to be structureless aggregations of classes of variables deemed relevant. As a sociological theorist he holds to a methodological individualism for sociology more dogmatically than would this reviewer. (E.g., p. 11: "The starting point for sociology is the behavior of individuals. Unless we understand the ways in which individuals behave we cannot hope to understand social processes and the functioning of groups.") Consequently his parameters of theory tend to be highly psychological. His psychology, of the perceptual-cognitive-phenomenological variety dominant in American social psychology today, includes "perceptions of the situation" as a causal factor in behavior, "believed future acts of other persons," "values," and "motivations." While recognizing the necessity of abstraction and of formal theories which can be applied only in experimentally controlled situations, he nonetheless shows in his own formulations the typical sociologist's nostalgia for an exhaustive understanding of the particular natural instance. This leads him to hopelessly unwieldy sets of variables.

Karlsson has covered for us a broad literature, and the general perspectives resulting are of value. Mathematical sociology is in its infancy. For any given

process, a subinfinity of mathematical models is available. While the discipline of data can help us select from among these, this process has not gone very far. No model is reported which has been successfully used on data other than that upon which it was developed. The empirical research which Karlsson

finds of interest has almost all been collected without the auspices of mathematical theory. We also learn that, unlike the rest of the European continent, Scandinavian social scientists are full participants in the developments which we in the English-speaking world find most exciting.

Lay Writers View Psychopathology

John Bartlow Martin

The Pane of Glass. New York: Harper & Brothers, 1959. Pp. xvi + 397. \$6.00.

Barbara O'Brien

Operators and Things: The Inner Life of a Schizophrenic. Cambridge, Mass.: Arlington Books, 1959 (distributed by Taplinger Publishing Co., New York). Pp. xii + 166. \$3.95.

Reviewed by RICHARD S. LAZARUS

Dr. Lazarus is a clinical psychologist, Professor of Psychology at the University of California in Berkeley, a specialist in the field of experimental psychodynamics, and continuing in its sixth year an experimental study of personality and stress supported by the National Institute of Mental Health. Two years ago he reviewed Mastery of Stress by Funkenstein, King, and Drolette (Harvard Univer. Press, 1957; CP, Mar. 1958, 3, 66f.).

THESE books have two things in common. They are written by lay persons and they deal with mental illness. Beyond this they diverge completely in style, purpose, and content. Barbara O'Brien describes a nightmarish six months of a paranoid schizophrenic episode during which she flees to an isolated cabin and wanders about the country on Greyhound buses. She plays host, in her mind, to a fascinating cast of hallucinated characters who appear to represent a synthesis of real people she knows and diverse qualities of her own unconscious. John Bartlow Martin, freelance writer of many documentary articles and books on social and political

subjects, attempts to give a picture of the field of mental illness in this country, especially reflected by the State Hospital at Columbus, Ohio, which the author presents as the prototype of the modern mental hospital.

Operators and Things is an absorbing story in its own right, independently of what it might show about a psychotic episode. The hallucinated characters (the "operators" who vie with each other for status and for control of Barbara's mind) appear as real persons of varied disposition who control "Things" (ordinary people) and interact with one another according to a set of rules of conduct. In the main they are portrayals of the inhumane and destructive qualities which Barbara perceives in the real-life people about her, whose machinations she fears so greatly that she finds some refuge in a temporary psychosis.

Aside from this story itself, the book must be judged in terms of its capacity to throw light on the processes of psychosis itself. The author has a smattering of knowledge about psychopathological theory which she interweaves with the account of her desperate struggle for psychological survival. It is thus

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THE NEUROPSYCHOLOGY OF LASHLEY

By FRANK A. BEACH, *University of California, Berkeley*; DONALD O. HEBB, *McGill University*; CLIFFORD T. MORGAN, *University of Wisconsin*; and HENRY NISSEN. Ready in May.

This volume, compiled by four of Lashley's former students, does honor to the memory of Karl S. Lashley—"the man who, as much as anyone, determined the shape of modern psychology." This is a compilation of Lashley's original papers, reflecting the range of his interest and achievements.

CONFLICT, AROUSAL, AND CURIOSITY

By D. E. BERLYNE. *McGraw-Hill Series in Psychology*. Ready in July.

An advanced, graduate-level book intended as a contribution to behavior theory. The author reviews such facts as are available relevant to attention and exploratory behavior; presents a tentative theory to integrate these facts and guide future research; and shows the connections between exploratory behavior and such areas as motivation and learning, thinking, aesthetics, and humor. Further, he points out interrelationships between various lines of theory and research in the field.

BARGAINING AND GROUP DECISION MAKING

By SIDNEY SIEGEL and LAWRENCE E. FOURAKER, *both of Pennsylvania State University*. In Press.

This work was the recipient of the Monograph Prize of the American Academy of Arts and Sciences for the year 1959 in the field of the social sciences. The book is the result of highly original work by a psychologist and an economist into the processes of decision making and choice, including investigation of psychological factors in economic decision. It reports a series of controlled experimental studies on the decisions individuals make when bargaining.

CRIME, JUSTICE, AND CORRECTION

By PAUL W. TAPPAN, *New York University*. *McGraw-Hill Series in Sociology*. Ready in May.

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impossible to know how much she is describing (and remembering) her experience and to what extent this knowledge has colored the account. Her fear and distrust of human society is clearly genuine, although its origin in the author's life history is never indicated. But her impressions of her illness and the interpretations of it which she assays seem at time to have been professionally guided or taken from a text.

Her identification of the "operators" who control her as her unconscious and her designation of "things" as her conscious mind (which in illness had lost its hold over its unconscious) has the quality of a clever secondary elaboration on a dream, an elaboration which arose not from her own direct insight but from current psychological lore. The tale is best when it is uninterrupted by these theoretical discourses. The dilemma of the researcher into psychopathology who reads the account is to separate what was really remembered of the experience from the semi-sophisticated editorializing. This is indeed a problem with all phenomenological approaches to psychological processes. After all, the literate organized narrative which is presented is always a far step from the raw data of Barbara's psychotic experience.

THE title, *The Pane of Glass*, is taken from a statement by a schizophrenic describing his sense of separation from mankind: "There is a pane of glass between me and mankind." In this volume the author has done a painstaking job of pulling together statistics about mental illness and mental hospitals from published data and by the direct interviewing of psychiatrists. The scope of the book is large in that it attempts to deal with the national picture, with the current status of research, and with the organization and functioning of various types of mental hospitals, but its major content is based upon a close inspection of the Columbus State Hospital. The author gives elaborate descriptions of the various wards, the types of patients residing in them, the psychiatrists who treat them, and their interactions with patients.

It is difficult to know for whom the book was written. Presumably it was aimed at educated laymen and profes-

sional persons whose function does not usually give them access to this kind of information—for example, general practitioners or medical specialists outside of psychiatry. For the layman, the book is probably too technical and tedious. Mr. Martin is a name and number dropper. One has the feeling that he has tried hard to impress the reader with the accuracy and scholarliness of his account, and that this is the reason for page after page of actual data about all manner of mental health matters, including numbers of patients and professional persons, money spent, frequency of different types of treatment, and so on. These figures, woven into the text when they appear relevant, increase the dullness and circumstantiality of the account. The descriptions of the activities on a ward also suffer, because the reader is given a bewildering panorama of names and brief case histories as a doctor makes ward rounds. There are no familiar characters with whom to feel at home. After the first few, further 'cases' tend to lull one to sleep. The collection of all these data with the accounts and descriptions of the participants is a prodigious accomplishment, but who is going to trudge through it all? The layman probably won't and the professional needn't.

How accurate a picture does one get of this field from Martin's extensive account? In many ways, the view is precise. It is also clear that most of the information was derived from interviews with a number of psychiatrists. The book thus presents their perspective, and this is clearly a slanted one. The author, not being a professional worker himself, is in no position to rise above this bias, and he reports essentially as the spokesman of a single profession, psychiatry.

Certainly one gets the impression that there is something heroic about the mental hospital psychiatrist, dedicated, always striving for understanding and to provide the very best of services, struggling against the terrible odds of public indifference and lack of funds. There is much ado about the insufficient number of psychiatrists, but at no point does the reader ever get the notion that there are other professions who also make the problem of mental health their business. Rather one gets

the feeling that even a poorly trained, second-rate psychiatrist is better than any nonpsychiatrist. The text makes almost no mention, for example, of clinical psychology and psychologists, and in the one clear case (a single line) where the term comes up, the psychologist is 'doing what he ought to do,' determining an intelligence level from a test.

THE same one-sidedness appears in the handling of the problem of research. Just about all of the ongoing work cited is being done by psychiatrists (even the tiny bit of nonpsychiatric research is misclassified in the bibliography), and most of this deals with drugs. In reading this account of mental-health research the reader cannot help but get the impression that the only hope for solving the problems of mental health lies in biochemistry (another specialty of psychiatry) and mostly with regard to the tranquilizing drugs. While there is some recognition of the controversy over this physiological orientation, the volume of material dealing with drugs is so overwhelming and the enthusiasm so great that it appears as if it were nearly the sole preoccupation of psychiatry.

It is clear that the author, as a very competent layman, depended exclusively for his information on the people he interviewed about the field. The impressions he got probably reflect the current views of the majority of psychiatrists. From some hospital wards the world may look as this book pictures it, but the over-all picture is something other.

All in all, these two books will not create a deep stir in the professions which deal with psychopathology. Of the two, *The Pane of Glass* is a serious and scholarly effort, and it does present the way psychiatry in general views the problem of mental health. It will rapidly become dated. *Operators and Things* is an interesting personal document which can be profitably added to the very small list of similar accounts. Perhaps more important, it provides for the author some indication of a promising career as a writer which may make her future task of adjusting to a threatening society a little easier.



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By MERTON M. GILL and MARGARET BRENNAN

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The authors apply psychoanalytic ego psychology to the interpretation of data gained in an extensive research project on hypnosis. They first re-examine their basic frame of reference, clarify some of its assumptions, and thereby make a genuine contribution not only to the understanding of the variety of hypnotic phenomena but also to the nature of regression.

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ON THE OTHER HAND



BLAME

Scriven's review of Sidney Hook's *Determinism and Freedom* (CP, Dec. 1959, 4, 380-382) holds that "responsibility is an absolutely necessary condition for punishment or reward." I think not. It is possible to control the behavior of organisms through reward or punishment without consideration of their "responsibility." And this may be as true of human beings as it is of rats.

Since the word *responsibility* carries a burden of assumption made impregnable by its emotional history, it may be advisable, if we know what public goals we seek through our moral and legal systems, to dispense with this concept as we have with other honorific but quarrelsome terms. (Cf. my Cruelty, dignity, and determinism, *Amer. Sociol. Rev.*, 1959, 24, 375-384.)

Scriven also writes that "the determinist cannot argue that one *should not blame*, for this is to argue that the act of blaming is *blameable*." This is cute, but, again, not quite accurate. No determinist I know of says, categorically, "Don't blame." Rather, blaming is discussed in the context of means toward specified ends. The determinist is concerned with the consequences for social policy of *assuming* free will and blame-worthiness. Thus, the determinist might say to Scriven, "It is unpsychological, or unscientific, to blame," and he could indicate how this is so. Or, better, he might say, "Blaming John for x-behavior under circumstances *abc* will probably have these *xyz*-results. Often, socially, these are not the effects you say you wish."

Punishment and reward may be viewed as re-directors of behavior. If they have this efficacy, they may be rationally applied—even to people who "could not have done otherwise."

GWYNN NETTLER
Mexico City

Nettler has two criticisms to make. The first rests on a misunderstanding. Naturally, I am not denying that one can inflict punishment without consideration of responsibility; in fact I stress it earlier in the para-

graph from which he quotes. I *am* denying that one can erect a *rational code* of punishment without requiring responsibility. Nettler denies this, too, in his last paragraph; but he provides no counter-examples and I think he has identified it with the other claim.

Nettler's second criticism is more interesting; he rejects my suggestion of self-refutation in the determinist's position. I must make it clear that determinism is not in general self-refuting; it is only in the exact form quoted under heading 4 that this charge can be sustained, if at all. It is thus of the greatest importance that we confine our intention to people who really believe in this version. Nettler dismisses this by saying he does not know any. In the sentence after the one of mine that he quotes I repeat the name of the man whose position I am discussing and who did say this: Clarence Darrow.

Determinists in general find it difficult to keep out of his difficulty; they want to be indignant about people who go around blaming others in *exactly* the way the people they attack are being indignant about the people they are blaming. This difficulty is hard to avoid, consistently, though I agree perfectly that the determinist is not inconsistent if he is only saying that blame is often *impolitic*. Alas, Nettler has to describe this as blame being *unscientific*. The trouble is that science is a game of truth, not of consequences, and it appears to be true that some people are to blame for some of their actions and hence scientific to say this (e.g., in distinguishing alcoholism from chronic drunkenness). Dropping the words will not make the difficulties go away, and I am not convinced that Nettler has succeeded in that task.

MICHAEL SCRIVEN
Swarthmore College

THE FAIR REVIEW

I have become disturbed by what appears to me to be a lack of responsibility on the part of some CP reviewers and wonder whether some change in editorial policy might help insure a more uniformly high quality of reviews.

Two reviews in recent months: Bronfen-

brenner's of Helen Lynd's *On Shame and Identity* (CP, Apr. 1959, 4, 114f.) and McCurdy's of Erik Erikson's *Young Man Luther* (CP, July 1959, 4, 201f.) have provoked considerable protest from the readers, based not only on different opinions as to the value of the books in question but also, and more disturbingly, pointing out many instances in which the reviewer had omitted vital information or simply distorted the author's meaning to fit his own point of view.

In the December issue of CP, Butler's review of Bernard Apfelbaum's *Dimensions of Transference in Psychotherapy* (4, 384) is a further, and glaring, example of such irresponsibility. None of the results of the study are cited in the review (although Butler himself admits that "many interesting results are reported") so that the reader is not allowed to discover what the study is really about or to obtain a context for evaluation of the reviewer's criticisms. Further, the derogation which characterized the review is based on idiosyncratic personal opinion. Since several theories of psychotherapy have developed, each stressing a different (and more or less measurable) conceptualization of transference, is it really 'unrealistic' to expect a study of transference to be based on other than a strictly Freudian viewpoint? Why should the fixed distribution of a Q-sort invalidate the interpretation of extreme placements as an index of strength of conviction? Should cluster analysis be dismissed simply as a "rough and ready technique," or should its appropriateness rather be evaluated as a method for the specific study, and in the context of the results it has afforded?

Butler expresses "little doubt" that factor analysis would have yielded "more reliable and *different* results." There can be no argument with the statement since even within a factor analytic approach different solutions are bound to yield different results. The question remains whether the results would have been more meaningful or useful; and, since the reviewer chooses to disregard the fact that Apfelbaum was able to isolate three meaningful clusters of patients which both differed on MMPI scales and showed varying responses to the experience of therapy, the reader is left in the position of being unable either to agree or to disagree with what is a crucial criticism of the study.

Butler seems to be using the review as a platform for airing his own prejudices rather than fulfilling a reviewer's obligation of evaluating theory and method within the specific context given by the

author, and of providing the reader with at least a minimal amount of unbiased information.

WANDA CHARWAT BRONSON
University of California at Berkeley

THE BEHAVIORIST'S BABY

Morris Bishop, since 1921 of Cornell's Department of Romance Languages, wrote the herebelow poem for the SatEvePost about 1927, the year Titchener died at Cornell; and CP reprints it because to CP's ear it is funny. In 1927 orthodoxy felt some aggression toward behaviorism, which was then heterodox, so the poem was releasing for some though frustrating for others. Anyhow let those who detect lèse majesté to the positivistic inheritor of psychology's throne, turn to something else.—Ed.

Only a baby small
Dropped from the skies,
Only a laughing face,
Two sunny eyes,
Waiting psychology's
Touch to attune it;
Only a wee
Biological Unit.

Sleep, little lily bud,
Guarded from fear;
Mamma is watching you,
Mamma is near.
Smiling so dreamily,
Tiny and slim you lie.
What a temptation for
Trying out stimuli!

What if you're suddenly
Dropped out of bed?
What of the pistol shots
Back of your head?
That's for your benefit;
What Mamma wants is
Just to condition her
Baby's responses.

Hushaby, babykin,
Why do you cry?
Why this malevolent
Gleam in your eye?
Good gracious! Mercy me!
See what he did?
He bit the behaviorist!
'At-a-boy, kid!

MORRIS BISHOP
Cornell University



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